SCIENCE

NEW SERIES
VOL. XLIV. No. 1137

FRIDAY, OCTOBER 13, 1916

SINGLE COPIES, 15 CTS.
ANNUAL SUBSCRIPTION, $5.00

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MSS. intended for publication and books, etc., intended for review should be sent to Professor J. McKeen Cattell, Garrison-on-Hudson, N. Y.

THE RELATION OF PURE SCIENCE TO INDUSTRIAL RESEARCH[1]

IT is not strange that many years ago Huxley, with his remarkable precision of thought and his admirable command of language, should have indicated his dissatisfaction with the terms "pure science" and "applied science," pointing out at the same time that what people call "applied science" is nothing but the application of pure science to particular classes of problems. The terms are still employed, possibly because, after all, they may be the best ones to use, or perhaps our ideas, to which these expressions are supposed to conform, have not yet become sufficiently definite to have called forth the right words.

It is not the purpose of this address, however, to suggest better words or expressions, but rather to direct attention to certain important relations between purely scientific research and industrial scientific research which are not yet sufficiently understood.

Because of the stupendous upheaval of the European war with its startling agencies of destruction—the product of both science and the industries—and because of the deplorable unpreparedness of our own country to defend itself against attack, there has begun a great awakening of our people. By bringing to their minds the brilliant achievements of the membership of this institute in electric lighting and power and communications and by calling their attention to the manifold

[1] President's address given at the thirty-third annual convention of the American Institute of Electrical Engineers.

achievements of the members of our sister societies in mechanical and mining and civil engineering, and the accomplishments of our fellow-workers, the industrial chemists, they are being aroused to the vital importance of the products of science in the national defense.

Arising out of this agitation comes a growing appreciation of the importance of industrial scientific research, not only as an aid to military defense but as an essential part of every industry in time of peace.

Industrial research, conducted in accordance with the principles of science, is no new thing in America. The department which is under my charge, founded nearly forty years ago to develop, with the aid of scientific men, the telephone art, has grown from small beginnings with but a few workers to a great institution employing hundreds of scientists and engineers, and it is generally acknowledged that it is largely owing to the industrial research thus conducted that the telephone achievements and developments in America have so greatly exceeded those of other countries.

With the development of electric lighting and electric power and electric traction which came after the invention of the telephone, industrial scientific research laboratories were founded by some of the larger electrical manufacturing concerns and these have attained a world-wide reputation. While vast sums are spent annually upon industrial research in these laboratories, I can say with authority that they return to the industries each year improvements in the art which, taken all together, have a value many times greater than the total cost of their production. Money expended in properly directed industrial research, conducted on scientific principles, is sure to bring to the industries a most generous return.

While many concerns in America now have well organized industrial research laboratories, particularly those engaged in metallurgy and dependent upon chemical processes, the manufacturers of our country as a whole have not yet learned of the benefits of industrial scientific research and how to avail themselves of it.

I consider that it is the high duty of our institute and of every member composing it, and that a similar duty rests upon all other engineering and scientific bodies in America, to impress upon the manufacturers of the United States the wonderful possibilities of economies in their processes and improvements in their products which are opened up by the discoveries in science. The way to realize these possibilities is through the medium of industrial research conducted in accordance with scientific principles. Once it is made clear to our manufacturers that industrial research pays, they will be sure to call to their aid men of scientific training to investigate their technical problems and to improve their processes. Those who are the first to avail themselves of the benefits of industrial research will obtain such a lead over their competitors that we may look forward to the time when the advantages of industrial research will be recognized by all.

Industrial scientific research departments can reach their highest development in those concerns doing the largest amount of business. While instances are not wanting where the large growth of the institution is the direct result of the care which is bestowed upon industrial research at a time when it was but a small concern, nevertheless conditions to-day are such that without cooperation among themselves the small concerns can not have the full benefits of industrial research, for no one among them is sufficiently strong to maintain the necessary staff and laboratories. Once the vital importance of this subject is appre-

ciated by the small manufacturers many solutions of the problem will promptly appear. One of these is for the manufacturer to take his problem to one of the industrial research laboratories already established for the purpose of serving those who can not afford a laboratory of their own. Other manufacturers doing the same, the financial encouragement received would enable the laboratories to extend and improve their facilities so that each of the small manufacturers who patronizes them would in course of time have the benefit of an institution similar to those maintained by our largest industrial concerns.

Thus, in accordance with the law of supply and demand, the small manufacturer may obtain the benefits of industrial research in the highest degree and the burden upon each manufacturer would be only in accordance with the use he made of it, and the entire cost of the laboratories would thus be borne by the industries as a whole, where the charge properly belongs. Many other projects are now being considered for the establishment of industrial research laboratories for those concerns which can not afford laboratories of their own, and in some of these cases the possible relation of these laboratories to our technical and engineering schools is being earnestly studied.

Until the manufacturers themselves are aroused to the necessity of action in the matter of industrial research there is no plan which can be devised that will result in the general establishment of research laboratories for the industries. But once their need is felt and their value appreciated and the demand for research facilities is put forth by the manufacturers themselves, research laboratories will spring up in all our great centers of industrial activity. Their number and character and size, and their method of operation and their relation to the technical and engineering schools, and the method of their working with the different industries, are all matters which involve many interesting problems—problems which I am sure will be solved as they present themselves and when their nature has been clearly apprehended.

In the present state of the world's development there is nothing which can do more to advance American industries than the adoption by our manufacturers generally of industrial research conducted on scientific principles. I am sure that if they can be made to appreciate the force of this statement, our manufacturers will rise to the occasion with all that energy and enterprise so characteristic of America.

So much has already been said and so much remains to be said urging upon us the importance of scientific research conducted for the sake of utility and for increasing the convenience and comfort of mankind, that there is danger of losing sight of another form of research which has for its primary object none of these things. I refer to pure scientific research.

In the minds of many there is confusion between industrial scientific research and this purely scientific research, particularly as the industrial research involves the use of advanced scientific methods and calls for the highest degree of scientific attainment. The confusion is worse because the same scientific principles and methods of investigation are frequently employed in each case and even the subject-matter under investigation may sometimes be identical.

The misunderstanding arises from considering only the subject-matter of the two classes of research. The distinction is to be found not in the subject-matter of the research, but in the motive.

The electrical engineer, let us say, finding a new and unexplained difficulty in the working of electric lamps, subjects the phe-

nomenon observed to a process of inquiry employing scientific methods, with a view to removing from the lamps an objectionable characteristic. The pure scientist at the same time investigates in precisely the same manner the same phenomenon, but with the purpose of obtaining an explanation of a physical occurrence, the nature of which can not be explained by known facts. Although these two researches are conducted in exactly the same manner, the one nevertheless comes under the head of industrial research and the other belongs to the domain of pure science. In the last analysis the distinction between pure scientific research and industrial scientific research is one of motive. Industrial research is always conducted with the purpose of accomplishing some utilitarian end. Pure scientific research is conducted with a philosophic purpose, for the discovery of truth, and for the advancement of the boundaries of human knowledge.

The investigator in pure science may be likened to the explorer who discovers new continents or islands or hitherto unknown territory. He is continually seeking to extend the boundaries of knowledge.

The investigator in industrial research may be compared to the pioneers who survey the newly discovered territory in the endeavor to locate its mineral resources, determine the extent of its forests, and the location of its arable land, and who in other ways precede the settlers and prepare for their occupation of the new country.

The work of the pure scientists is conducted without any utilitarian motive, for, as Huxley says, "that which stirs their pulses is the love of knowledge and the joy of discovery of the causes of things sung by the old poet—the supreme delight of extending the realm of law and order ever farther towards the unattainable goals of the infinitely great and the infinitely small, between which our little race of life is run." While a single discovery in pure science when considered with reference to any particular branch of industry may not appear to be of appreciable benefit, yet when interpreted by the industrial scientist, with whom I class the engineer and the industrial chemist, and when adapted to practical uses by them, the contributions of pure science as a whole become of incalculable value to all the industries.

I do not say this because a new incentive is necessary for the pure scientist, for in him there must be some of the divine spark and for him there is no higher motive than the search for the truth itself. But surely this motive must be intensified by the knowledge that when the search is rewarded there is sure to be found, sooner or later, in the truth which has been discovered, the seeds of future great inventions which will increase the comfort and convenience and alleviate the sufferings of mankind.

By all who study the subject, it will be found that while the discoveries of the pure scientist are of the greatest importance to the higher interests of mankind, their practical benefits, though certain, are usually indirect, intangible or remote. Pure scientific research unlike industrial scientific research can not support itself by direct pecuniary returns from its discoveries.

The practical benefits which may be immediately and directly traced to industrial research, when it is properly conducted, are so great that when their importance is more generally recognized industrial research will not lack the most generous encouragement and support. Indeed, unless industrial research abundantly supports itself it will have failed of its purpose.

But who is to support the researches of the pure scientist, and who is to furnish him with encouragement and assistance to pur-

sue his self-sacrificing and arduous quest for that truth which is certain as time goes on to bring in its train so many blessings to mankind? Who is to furnish the laboratories, the funds for apparatus and for traveling and for foreign study?

Because of the extraordinary practical results which have been attained by scientifically trained men working in the industrial laboratories and because of the limited and narrow conditions under which many scientific investigators have sometimes been compelled to work in universities, it has been suggested that perhaps the theater of scientific research might be shifted from the university to the great industrial laboratories which have already grown up or to the even greater ones which the future is bound to bring forth. But we can dismiss this suggestion as being unworthy.

Organizations and institutions of many kinds are engaged in pure scientific research and they should receive every encouragement, but the natural home of pure science and of pure scientific research is to be found in the university, from which it can not pass. It is a high function of the universities to make advances in science, to test new scientific discoveries and to place their stamp of truth upon those which are found to be pure. In this way only can they determine what shall be taught as scientific truth to those who, relying upon their authority, come to them for knowledge and believe what they teach.

Instead of abdicating in their favor, may not our universities, stimulated by the wonderful achievements of these industrial laboratories, find a way to advance the conduct of their own pure scientific research, the grand responsibility for which rests upon them. This responsibility should now be felt more heavily than ever by our American universities, not only because the tragedy of the great war has caused the destruction of European institutions of learning, but because even a worse thing has happened. So great have been the fatalities of the war that the universities of the old world hardly dare to count their dead.

But what can the American universities do, for they, like the pure scientists, are not engaged in a lucrative occupation. Universities are not money-making institutions, and what can be done without money?

There is much that can be done without money. The most important and most fundamental factor in scientific research is the mind of a man suitably endowed by nature. Unless the scientific investigator has the proper genius for his work, no amount of financial assistance, no apparatus or laboratories, however complete, and no foreign travel and study, however extensive, will enable such a mind to discover new truths or to inspire others to do so. Judgment and appreciation and insight into character on the part of the responsible university authorities must be applied to the problem, so that when the man with the required mental attributes does appear he may be appreciated as early in his career as possible. This is a very difficult thing to do indeed. Any one can recognize such a man after his great achievements have become known to all the world, but I sometimes think that one who can select early a man who has within him the making of the scientific discoverer must have been himself fired with a little of the divine spark. Such surely was the case with Sir Humphry Davy, himself a great discoverer, who, realizing the fundamental importance of the man in scientific discovery, once said that Michael Faraday, whose genius he was prompt to recognize, constituted his greatest discovery.

I can furnish no formula for the identi-

fication of budding genius and I have no ready-made plan to lay before the universities for the advancement of pure scientific research. But as a representative of engineering and industrial research, having testified to the great value of pure scientific research, I venture to suggest that the university authorities themselves might well consider the immense debt which engineering and the industries and transportation and communications and commerce owe to pure science, and to express the hope that the importance of pure scientific research will be more fully appreciated both within the university and without, for then will come—and then only—that sympathetic appreciation and generous financial support so much needed for the advancement of pure scientific research in America.

While there are many things—and most important things—which the universities can do to aid pure science without the employment of large sums of money, there are nevertheless a great many things required in the conduct of pure scientific research which can be done only with the aid of money. The first of these I think is this:

When a master scientist does appear and has made himself known by his discoveries, then he should be provided with all of the resources and facilities and assistants that he can effectively employ, so that the range of his genius will in no way be restricted for the want of anything which money can provide.

Every reasonable and even generous provision should be made for all workers in pure science, even though their reputations have not yet become great by their discoveries, for it should be remembered that the road to great discoveries is long and discouraging and that for one great achievement in science we must expect numberless failures.

I would not restrict these workers in pure science to our great universities, for I believe that they should be located also at our technical schools, even at those with the most practical aims. In such schools the influence of a discoverer in science would serve as a balance to the practical curriculum and familiarize the student with the high ideals of the pure scientist and with his rigorous methods of investigation. Furthermore, the time has come when our technical schools must supply in largely increasing numbers men thoroughly grounded in the scientific method of investigation for the work of industrial research.

Even the engineering student, who has no thoughts of industrial research, will profit by his association with the work of the pure scientist, for if he expects ever to tread the higher walks of the engineering profession he must be qualified to investigate new problems in engineering and devise methods for their solution and for such work a knowledge of the logical processes of the pure scientist and his rigorous methods of analyzing and weighing evidence in his scrupulous search for the truth will be of the greatest value.

Furthermore, the engineering student should be taught to appreciate the ultimate great practical importance of the results of pure scientific investigation and to realize that pure science furnishes to engineering the raw material, so to speak, which he must work into useful forms. He should be taught that after graduation it will be most helpful to him and even necessary, if he is to be a leader, to watch with care the work of the pure scientist and to scrutinize the reports of new scientific discoveries to see what they may contain that can be applied to useful purposes and more particularly to problems of his own which require solution. There are many unsolved problems in applied science, to-day, which are insoluble in the present state of our knowl-

edge, but I am sure that in the future, as has so often happened in the past, these problems will find a ready solution in the light of pure scientific discoveries yet to be made. When thus regarded the work of the pure scientist should be followed with most intense interest by all of those engaged in the application of science to industrial purposes. Acquaintance, therefore with the pure scientist, with his methods and results, is of great importance to the student of applied science. I believe that there is need of a better understanding of the relations between the pure scientist and the applied scientist and that this understanding would be greatly helped by a closer association between the pure scientist and the students in the technical schools.

While I have drawn a valid distinction between the work of the two, they nevertheless have much in common. Both are concerned with the truth of things, one to discover new truths and the other to apply these truths to the uses of man. While the object of the engineer is to produce from scientific discoveries useful results, these results are for the benefit of others. They are dedicated to the use of mankind and, as is the case with the pure scientist, they should not be confused with the pecuniary compensation which the engineer himself may receive for his work for this compensation is slight, often infinitesimally so, compared with the great benefits received by others. Like the worker in pure science, the engineer finds inspiration in the desire for achievement and his real reward is found in the knowledge of the benefits which others receive from his work.

There are many other things which might be discussed concerning the conduct of pure scientific research in our universities and technical schools, but enough has been said to make it plain that I believe such work should be greatly extended in all of our American universities and technical institutions. But where are the universities to obtain the money necessary for the carrying out of a grand scheme of scientific research? It should come from those generous and public-spirited men and women who desire to dispose of their wealth in a manner well calculated to advance the welfare of mankind, and it should come from the industries themselves, which owe such a heavy debt to science. While it can not be shown that the contribution of any one manufacturer or corporation to a particular purely scientific research will bring any return to the contributor or to others, it is certain that contributions by the manufacturers in general and by the industrial corporations to pure scientific research, as a whole, will in the long run bring manifold returns through the medium of industrial research conducted in the rich and virgin territory discovered by the scientific explorer.

It was Michael Faraday, one of the greatest of the workers in pure science, who in the last century discovered the principle of the dynamo electric machine. Without a knowledge of this principle discovered by Faraday the whole art of electrical engineering as we know it to-day could not exist and civilization would have been deprived of those inestimable benefits which have resulted from the work of the members of this institute.

Not only Faraday in England, but Joseph Henry in our own country and scores of other workers in pure science have laid the foundations upon which the electrical engineer has reared such a magnificent structure.

What is true of the electrical art is also true of all the other arts and applied sciences. They are all based upon fundamental discoveries made by workers in pure science, who were seeking only to discover

the laws of nature and extend the realm of human knowledge.

By every means in our power, therefore, let us show our appreciation of pure science and let us forward the work of the pure scientists, for they are the advance guard of civilization. They point the way which we must follow. Let us arouse the people of our country to the wonderful possibilities of scientific discovery and to the responsibility to support it which rests upon them and I am sure that they will respond generously and effectively. Then I am confident that in the future the members of this institute, together with their colleagues in all of the other branches of engineering and applied science, as well as the physician and surgeon, by utilizing the discoveries of pure science yet to be made, will develop without marvelous new agencies for the comfort and convenience of man and for the alleviation of human suffering. These, gentlemen, are some of the considerations which have led me here in my presidential address to urge upon you the importance of a proper understanding of the relations between pure science and industrial research.

J. J. Carty

THE BOTANICAL FIELD EXCURSION IN COLLEGIATE WORK

The standard college course in general botany occupies a well-defined field, and is concerned with pedagogical problems quite distinct from those of the secondary school on the one hand, and of the university on the other. Many of the defects and shortcomings of collegiate botany as taught have been due to the fallacious idea that college botany is merely university botany pruned down to meet the supposititious mental ability of the college student. The ideas and technique of the university research laboratory have frequently been transplanted *en bloc* into the college classroom, with resultant pedagogic malpractise and scientific inefficiency.

College and university men are coming to realize more and more clearly that the university research laboratory has its peculiar problems, for which work it should be diligently protected and fostered; and also that the American college as an institution has its distinctive field and problems, and that the two fields, overlapping here and there, are on the whole widely separated from one another.

One of the notable lines of weakness of the collegiate course in general botany that has come to the writer's attention, is the comparatively rare or infrequent use of the field excursion. The usual schedule, to be found in most American colleges, consists of one or two trips in the autumn, a long winter session restricted almost exclusively to laboratory exercises, and a few desultory spring trips to collect flowering plants.

There are a number of factors which have combined to bring about this state of affairs. Most botany teachers are primarily laboratory-trained men. Frequently they are not very well acquainted with the region in which they teach. In many instances their own university work in botany was confined largely to the cytological, histological or morphological aspects of the science; with little or no practical training in field work, either from the scientific or pedagogical viewpoint. In most regions a large portion of the academic year is winter time, with much inclement weather, and plant life at a standstill. Laboratory exercises can be planned with much greater certainty and precision than can field trips. The problems of transportation and discipline on the field trip, particularly if the class be large, are often difficult and annoying. It involves much planning and extra work to break up a large class into small sections for field work. Field trips are time consuming, and in many regions the places of greatest botanic interest lie at a considerable distance from the college buildings. There are a great number of excellent printed outlines covering all the standard laboratory exercises and experiments; these laboratory guides and manuals are ready-made for the teacher's use, while field trips require the laborious preparation of special outlines by the

individual teacher.[1] For all these reasons, and for others that might be enumerated, the average college teacher finds it much easier, and on the whole more satisfactory to plan laboratory exercises rather than field excursions.

The present paper is an earnest plea for a larger recognition of field work as an *integral part* of any course in general botany. The field work should not usurp the place of legitimate laboratory studies, but on the other hand it should not be regarded, as it is generally regarded to-day, as a mere accessory, desirable but inconvenient. Ganong's statement may be appropriately quoted here:

> Very important too, are field excursions, the opportunity for which varies greatly. Theoretically, it might seem better if most botanical study could be done out of doors, but practically the greater part of it demands tools and other facilities, including physical comfort, unobtainable away from a good laboratory. In the excursions the teacher will of course direct attention to the larger phenomena of adaptation, the topography or physiognomy of the vegetation, the plant associations, etc. This kind of study will become much easier and more profitable in the near future as the subject becomes more fully systematized, and good books on it become accessible. It is especially important not to allow too great a number of students to go together on these excursions, and in my own experience not over ten can be profitably taken at any one time. The collecting instinct, so invaluable to the naturalist, should at such times receive every possible encouragement.[2]

Botany exists first of all out-of-doors, and the college student should have thoroughgoing training in field work as well as in the laboratory, herbarium and library. The college student, interested primarily in the large, significant, dynamic aspects of the subject, rather than in technical minutiæ, should be deeply imbued with the idea that he is working with an *out-of-door* subject, and that a valuable and essential part of the course is his own training in actual observation of *live* plants.

A pedagogical mistake that characterizes much botanic field work is the failure to place sufficient emphasis upon the vital, ecologic aspects of the studies. As Trafton[3] states,

> The demand for the study of physiology and ecology are protests against the old methods of looking on plants as lifeless things to be analyzed, classified, and laid away like minerals. It is insisted that the student shall be taught to look on plants as possessing life just as truly as do animals, and as having life problems to solve.

All too easily may a trip become a mere dilettante wandering, a grubbing up of plants, a hasty confusion of botanic names, a rude packing of specimens for herbarium or laboratory purposes. The essence of field work is to observe the plant *in its environment*, and to reason scientifically from these observations. As Adams[4] succinctly remarks,

> To learn how to study in the field, and not simply to collect, is one of the most important habits which a field naturalist and the ecologist has to acquire. This is one which he must, to a large degree, master alone, without the ready access to assistance, as is usually the case in the laboratory study. It is also a subject about which it is difficult to give useful suggestions, other than those of the most general character.

The herbalistic or laboratory routine, no matter how scientific and thoroughgoing, can never be more than a weak and shadowy substitute for these fundamental studies of organism and environment. Botany is not primarily in a *room*, it is out-of-doors; the workroom with its equipment and library is an adjunct to nature, and *not* the reverse. How often one finds botany taught as though the field and woodlands were merely a sort of glorified greenhouse, from which a few "types" and "illustrative specimens" were to be culled. Some teachers unconsciously create the impression that the plant kingdom exists primarily for the

[1] As an example of a recent text that does give suggestions for field work, E. F. Andrews, "Practical Botany," American Book Co., 1911, may be cited. Each of the ten chapters concludes with an excellent concise and suggestive section on field studies.

[2] Ganong, W. F., "The Teaching Botanist," Macmillan, 1899, pp. 64–65.

[3] Trafton, G. H., "Comparison of Methods of Teaching Botany," *School Review*, Vol. 10, 1902 (Feb.), pp. 138–145.

[4] Adams, C. C., "Guide to the Study of Animal Ecology," Macmillan, 1913, p. 37, Chap. 3, deals with field study.

purpose of providing material for paraffin sections and balsam mounts. To give college students real knowledge of plant life one must use living plants, and not merely skeletons and sections, no matter how important the latter are in their way.

As a concrete illustration of a general course in college botany that is given in an environment unusually favorable for field work, the writer will refer to the College of Hawaii, Honolulu. This institution corresponds in general status and organization to the state universities upon the mainland. Honolulu enjoys remarkably equable weather throughout the year; there are no storms; no frost, snow, ice or hail; thunder and lightning are very rare. There is no marked dormant season, and very few deciduous plants. The forests are evergreen, and most of the seed-plants have prolonged flowering periods. The climatic conditions are practically ideal for field work. In the immediate vicinity of the college is a remarkable variety of ecologic zones and habitats, ranging from the abyssal ocean to mountain peaks of three thousand feet elevation.

The botany course referred to is a freshman subject. There are two afternoon periods—two and one half hours each—and one lecture period per week. Customarily one of the afternoon periods is used for field work, the other for laboratory work. There are thirty-six weeks in the college year. The total number of field trips made by the class as a whole is about thirty. Students are encouraged to do individual field work and collecting, either on assigned topics, or those of their own choosing. This encourages the botanically-inclined student to develop a taste for original observations, and often prepares the way for special studies of genuine scientific merit.

The trips usually occur on Monday afternoon, as the experience of several years has proved this time to be the most satisfactory in connection with other features of the week's schedule. This permits the keeping-over of material collected, for the laboratory period, and facilitates a close coordination between field and laboratory work. The official period is two and one half hours, but the distances covered by some of the trips necessitate a considerably longer time than this, and field periods of three or three and one half hours are not uncommon. Occasionally, for the purpose of visiting some distant region of special interest, a double period is arranged by mutual agreement, and the excursion will occupy a period of five or six hours. On these occasions each student brings a light lunch, which is eaten at some convenient time in the course of the trip.

There are several types of excursions, which may be classed as follows:

1. *Systematic Collecting.*—To study in the field and collect for laboratory examination the plants of a given group or region; *e. g.*, green algæ; lichens; lycopods; Leguminosæ; strand plants; stream plants; swamp plants It is almost needless to point out that a certain amount of systematic collecting naturally forms a part of any field trip, irrespective of other objects.

2. *Ecologic Studies.*—Field studies of well-defined ecologic factors and adaptations; habitats with strongly marked characteristics; studies of zonation, invasion, competition, succession, etc.; relations of plant organs to environmental factors.[5]

3. *Field Studies of Plant Members and Organs.*—Particularly those organs and structures that are not adapted to bringing into the laboratory, *e. g.*, plank-roots, buttress roots; trunk types; bamboo; lianas *in situ;* epiphytes *in situ;* palm inflorescences; and many flowers.

4. *Phytogeographic Studies.*—Floral zones and regions in relation to their physiographic background; distinctive plants of the coral reef, lagoon, littoral, lowlands, valleys, summit ridges, peaks, etc.

[5] "On ecology of the structures they—the students—can do little better than guess at uses; for removed from their native homes, the plants can give no idea of their habits. Here is where the outdoor study of native plants through field excursions is most valuable." Ganong, *loc. cit.*, p. 206.

"Early in field work one should learn that the collection of specimens is not the primary aim of excursions, that specimens are only *one* kind of facts." Adams, *loc. cit.*, p. 41.

5. *Representative Plants.*—The phyla; important orders, families and genera; typical economics and ornamentals.

In all of the trips the students are encouraged to note any plants that are unknown to them. These are identified and listed; the last pages of the field notebook are utilized for this reference list of common plants. By this method, in the course of the year, the students learn the names of practically all the common plants of the region. As Clute cogently states,

> . . . the identification of plants is the only phase of botany in which the general public is interested; it is frequently the only part of botany in which the pupil is interested; and it is certainly the only part of botany that he follows up after he has left school. Doubtless every teacher has remarked the surprise of pupils when they discover that botany is not chiefly concerned with the names of plants.
>
> In any study, however, we can not do much without knowing the names of the objects with which we deal. Possibly there would be a much larger percentage of the people permanently interested in botany if our school courses early took cognizance of the desire for the names of things.[6]

At the beginning of the trip each student is provided with a typewritten or mimeographed outline which contains the essential topics, questions and directions for the trip. The topics and questions are numbered consecutively on the sheets, throughout the year, and the student numbers the paragraphs of his record to correspond with those of the outline. Inasmuch as the essential purpose of the trip is to strongly emphasize *individual observations* and first-hand familiarity with field material, the topics and questions are specifically planned and phrased with this object in view.

It is the practise of the teacher to devote a period of ten or fifteen minutes, early in the course of the trip, to a detailed explanation of the outline, so that every student knows exactly the character of the observations and studies to be made. At this time any questions are answered, individual assignments made, and every effort made to have the plan for the day thoroughly understood. This is a matter of great importance, as much time can be wasted through students not knowing exactly what is expected of them.

At this point it may be stated that each field trip is definitely anticipated in the lecture and recitation work, and much of the material and observations resulting from the trip are immediately used in the succeeding periods. The field trip is an *integral* working part of the course, and not merely a pleasant adjunct.

Some teachers utilize a somewhat less formal type of trip,[7] but it has been the writer's experience that the scientific results of an excursion are invariably in direct proportion to the fullness and precision of the outline.

An essential part of the equipment of each student is the *field notebook.* This is a small book, 3½ by 5 inches, with durable board covers and ordinary record ruling. In this book all of the original field notes and records are made, usually in pencil, and following the outline supplied at the beginning of each trip. The student numbers the pages consecutively, and the records appear in chronological sequence. At the end of the course a simple index is prepared by the student, listing the trips by subjects and places, and referring to the numbered pages of the book. The index is written on the first few pages of the book, which are left vacant for that purpose.

Much attention is given to the field notes as the record of the student's individual observations. A concise, simple style is encouraged. Technical terms are used when necessary, in a normal way, with no effort either to evade or to exaggerate their importance. Simple outline sketches, sections, profiles, diagrams and maps are used wherever they have

[6] Clute, W. N., "Teaching the Names of Animals and Plants," *School Review,* Vol. 15, 1907 (June), pp. 463–66.

[7] Clute, W. N., "Making Botany Attractive," *School Review,* Vol. 17, 1909 (Feb.), pp. 97–98. "Field trips are frequent, even in cold weather. Some trips are simply in quest of material and are made without an outline. Pupils are required to collect their own material and to note its relation to its surroundings and habitat. The trips with outlines are for the study of some phase of botany that can not well be studied indoors."

a legitimate place; the field drawings are necessarily rough, and are used, never for their own sake, but to supplement and elucidate the written statement.

It has been the experience of the writer that the freshman college student has a very vague idea as to essentials and non-essentials in field work and field records, and must be given systematic training in this. The records for the first few trips are examined by the teacher with particular care, and fully criticized and corrected. This is usually sufficient to give the student an accurate idea of standard field records. Insistence is placed upon the principle that field work must be genuine *field* work, and a rigid *tabu* is placed upon the writing up of notes from memory days after the trip has occurred. Two excerpts from Adams's[8] lucid statement may be pertinently reproduced herewith:

> The processes of observation and field study and note-taking are so intimately related that taking notes becomes one of the essential parts of careful observation. This is also one of the most difficult habits to acquire. The beginner is inclined to write them up, especially field notes, in the evening after his return from the field. Such notes are generally brief, lack details, and are usually of little value.
>
> We sometimes hear that reflections upon the work should be reserved for the return to the laboratory or study. This advice seems to be based on the assumption that study in the field is not particularly stimulating and suggestive. On the other hand, deliberating interpretatively in the midst of the problems under consideration is one of the most favorable conditions possible for the improvement of the quality and quantity of one's work.

A number of articles of field equipment are habitually taken on the trip, and are listed herewith.[9] Not all of these are taken on every trip; the kit is modified from time to time to suit the particular needs of the day.

[8] Adams, C. C., *loc. cit.*, pp. 41–42.

[9] The little book "Botanizing," by W. W. Bailey, Preston and Rounds, Providence, 1899, contains much useful information concerning botanical field work; especially Chap. 2, on equipment, Chap. 3, on collecting, and Chap. 4, giving directions for particular families.

1. *Vascula*—a number of small ones, one to each student, or to every two or three students, depending upon the character of the collecting; frequently one or two large vascula, for woody specimens or other bulky material.
2. *Diggers or trowels*—one or more, depending upon nature of collecting; the entomological collecting-tool listed by Kny-Scheerer has proved particularly satisfactory. Narrow garden trowels are good.
3. *Pocket-knives*—several large, strong knives, with sharp blades. The writer has been surprised and amused many times by the pocket-knife equipment of the average college student; the girls have none, and those of the men are usually wholly unsuitable for botanical purposes. Students are encouraged to supply themselves with good substantial pocket-knives, for as every field botanist knows, a surprising amount of botanical dissecting and anatomical work can be done with an ordinary *sharp* knife and a Coddington lens.
4. *Coddington lens*—one inch, in folding metal case; one for each student.
5. *Maps*—of the region to be visited, the largest scale obtainable; giving topography, hydrography, etc.
6. *Magnetic compass*—for use in connection with map work.
7. *Compound microscope*—portable type; occasionally taken, to provide for the demonstration, in the field, of certain structures —*e. g.*, algæ, fungi, sporangia, prothalli, elaters, pollen, protonema, etc.
8. *Dissecting kit*—in folding leather pocket-case, with scalpels, scissors, needles, etc., for field dissections.
9. *Field glasses*—prism binoculars; used on trips into the mountainous districts.
10. *Steel tape*—K. & E. 50 ft. Lilliput, very light and convenient; English and metric graduations; useful in many ways.

Miscellaneous vials, tin boxes, paper envelopes, twine, gummed labels, etc., as occasion requires.

For studies along the coral reefs and beaches waterboxes and collecting pails are taken.

Occasionally photographic equipment is taken, although this is made distinctly subsidiary to the other work of the trip.

A feature of the equipment that is by no means negligible is the item of clothing. Announcement of the trip is made a number of days, sometimes a full week, before the specified date, in order that all members of the class may have ample time to make any individual arrangements necessary. A statement is also made as to the general itinerary, the character of the country to be traversed, and the general nature of the garb most suitable for the trip.

Nothing is more disastrous to the pedagogical success of a trip than to have students appear in ill-adapted or wholly unsuitable clothing. One simply *can not* botanize in "good" clothes. Khaki trousers or skirts; headgear not susceptible to injury by the weather; leggins or puttees for protection against the numerous thorny and spiny plants of our lowlands; and, most important of all, comfortable, thick-soled, wide-heeled shoes—these are some of the features that make for successful field work. French heels, umbrellas and "wraps" are *tabu,* but the students are encouraged to bring field glasses, kodaks, or other equipment in addition to the botanical equipment, that will add to the interest of the trip.

In the first year course in general botany given at the College of Hawaii the following representative ecologic districts are visited:

1. *The Coral Reefs.*—This includes not only a survey of the plant life of the reef, but also a general study of reef formation; the reef as a habitat for plants and animals; the interrelations of marine organisms; the zonation of the reef and its waters; the rôle of plants as reef builders.

2. *The Beach.*—This includes the plant life of coral, tufa and lava beaches; the relation of plants to wave action; beach zonation; drift material; dissemination of plants by ocean currents; effects upon plant life of elevation and subsidence of beach levels; beach halophytism and xerophytism.

3. *The Lowlands.*—Comprising a variety of habitats—grassy plains; arid and semi-arid wastelands; salt-, brackish- and fresh-water swamps; streams and wet-lands; elevated limestone platforms; lava flows in various stages of disintegration; tufa cones and deposits; plant formations on volcanic ash and scoria.

Particular attention is given to the lowland flora, for although it is composed chiefly of introduced plants, it is the region in which the human population exists, and is therefore of chief interest. Problems of invasion, competition, adaptation, succession; dissemination; interrelations of insects and fungi to common lowland plants; crops, fruits, ornamentals and other economics; studies in xerophytism, mesophytism, etc.

4. *The Forest Zone.*—There are three divisions of the forest zone, lower, middle and upper; each of these has distinct humid and arid sections, with intergrading districts. In the vicinity of Honolulu only the lower and middle zones exist; the upper zone is confined to the lofty mountains of Maui and Hawaii. The forests within reach of Honolulu are chiefly humid or "rain" forests, although there are some xerophytic species. Topics: The conspicuous trees, shrubs and herbaceous plants of the forest; the forest as a watershed; the forest floor; animal life of the fòrest;[10] lianas and other specialized stem forms; precinctive species and varieties; landslides and other destructive agencies; relation of forest to precipitation, wind, elevation, etc.; conspicuous forest flowers and fruits; changes in the native forest within historic times; planted forests; forest conservation.

5. *Valleys.*—The Oahu Mountains are deeply dissected by steep-walled valleys, ravines and gorges. Many of these valleys are great amphitheaters of erosion. The humidity increases

[10] "Let no one worry if zoology and physical geography creep in hodge-podge with botany. They are apt to do that out of doors. Flowers do not object to the birds singing above them; I think an old tree likes to harbor a squirrel; and as for the boy who can gather spirogyra and not see a peculiar stone close by, he will never make a great naturalist."—Stuart, M. H., 1908, "The Botany Notebook, What it Should Contain and How it Should Be Made," *N. E. A. Proc.*, 1908, pp. 665–67.

progressively and conspicuously from mouth to head. Topics: plant zonation of the valley floor and walls; plant life of the stream and its borders; plants of precipices, spurs, hanging valleys and summit ridges.

The first trips of the course are short—across the college farm, and in the immediate vicinity—to familiarize the students with the general plans and methods of field work. The longest trips come late in the school year, after the class is thoroughly accustomed to field collecting and the ecologic point of view.

TYPICAL FIELD RECORD OUTLINE FOR THE STUDY OF A SEED-PLANT

1. *Name of plant*—Scientific; English; Hawaiian.
2. *Family.*
3. *Location*—as specific as feasible. (Students need training in accurate designation of localities.)
4. *Habitat*—distinctive features; soil; moisture; exposure; elevation; shade; plant associates; etc.
5. *The stand*—solitary; clumps; extensive pure stands; colonial; etc.
6. *Growth-form and duration*—herb; vine; shrub; tree; rosette; prostrate, etc.; outline sketch of profile, drawn to scale.
7. *Stem*—dimensions; characteristics of bark; mode of branching; cross-section of stem; mode of growth; special features and adaptations of stem, water-storage, etc.
8. *Foliage*—phyllotaxy; light relation. Description of leaf: blade—shape, size, color, texture, venation, apex, base, margin, other features; petiole—length, cross-section, etc. Collect six typical leaves for laboratory work. Variation; polymorphism; accessory structures. Leaf fall; leaf scars.
9. *Inflorescence*—abundance; location; kind; season.
10. *The flower*—color; odor; shape; size; flower buds; special features, nectaries, etc. Collect six flowers, in various stages of development, for laboratory work. Pack carefully to avoid crushing. Pollination—method and agents; desirable and undesirable insect visitors; protection of pollen from rain, etc., close-pollination.
11. *Fruit*—abundance; kind; size; shape; color; texture; dissemination. Collect six fruits in various stages of growth, for anatomical studies of fruits and seeds in laboratory.
12. *Seeds*—abundance; size; shape; color; dissemination.
13. Examine the various parts and organs of the plant for fungous diseases and insect pests, malformations, etc. Collect plenty of material for laboratory work.
14. *Root system*—if practicable, dig up several plants and determine character of roots, and area occupied by them. (Studies of roots in the field are very important, and constitute a much-neglected phase of botanical teaching. The writer strongly believes that at some point in the course the student should himself dig up and carefully examine the root systems of several representative plants.)
15. *Relations* of the individual plant to its associates—competition; commensalism; stratification; succession; etc.; visible evidence of adaptation.

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"EXPEDITE THE MAP"

A committee to "Expedite the Completion of the Topographic Map of the United States" has been formed on the invitation of the undersigned, and its circulars have lately been sent out to engineers and others in all parts of the country, asking their support of the movement. Although appropriations made by congress have been liberal, although a number of states have cooperated generously, and although topographic mapping has been industriously prosecuted by the U. S. Geological Survey for the last thirty-five years, only about 40 per cent. of our national domain is at present represented on standard topographic maps. The area annually covered was greater at first, when the work was less accurate, than now, since the demand for better maps has arisen: at the present rate, about a century will be required to complete the maps, and long before that time elapses the demand for maps of larger scale and still greater accuracy will retard the rate of progress, unless large funds are forthcoming. For ten years past, something over half a million dollars has been spent annually on field work alone. This large sum should be steadily increased until it is at least doubled, in order that too great a delay before maps of the whole country are available shall be avoided. A rapid increase in appropriations is not desirable, because only a relatively small number of trained topographers are available for the work; but the increase should be continued annually for some ten or fifteen years to come.

Every industry, art and science which demands a knowledge of the lay of the land is benefited by good maps of the area in which it is carried on. The general location of railways and highways, the planning of water-supply, irrigation and drainage projects, the prosecution of geological, soil and forest surveys, the development of water powers and the installation of electric transmission lines, the promotion of large-scale realty transactions such as are common in the less settled parts of the country, are all aided immensely if good topographic maps of their areas are available, and are correspondingly embarrassed if such maps are wanting. Practical men, who have had experience in mapped and in unmapped areas, can testify to the ease and the difficulty of work in the two cases.

It is the wish of the committee to secure letters from such men in all parts of the country as to the value of the maps in the surveyed areas and as to the need of maps in the unsurveyed areas. The testimony thus gathered will be submitted to the director of the U. S. Geological Survey, as the basis of an urgent request that he should ask for larger appro-

priations for topographic work; and if he does so, the correspondents of the committee to "expedite the map" will be requested to appeal to their congressmen in support of the director's budget. Readers of SCIENCE who have experience regarding the value and the need of maps are urged to take part in this campaign by writing to the secretary of the committee, Professor A. E. Burton, Massachusetts Institute of Technology, Cambridge, Mass.

The other members of the committee are Robert Bacon, president, National Security League, New York; Arthur H. Blanchard, consulting engineer, National Highway Association, professor of highway engineering, Columbia University, New York; G. P. Coleman, state commissioner of highways, Richmond, Va.; G. E. Condra, president, National Conservation Congress, State University, Lincoln, Nebr.; W. L. Darling, chief engineer, Northern Pacific Railway Company, St. Paul, Minn.; R. E. Dodge, president, National Council Geography Teachers, Teachers College, New York; A. B. Fletcher, state highway engineer, Sacramento, Calif.; W. Cameron Forbes, of J. M. Forbes and Co., Boston, Mass.; John R. Freeman, consulting engineer, Providence, R. I.; W. O. Hotchkiss, state geologist, Madison, Wis.; F. H. Newell, professor of civil engineering, University of Illinois, Urbana, Ill.; Joseph H. Pratt, state geologist, Chapel Hill, N. C.; Wm. Barclay Parsons, consulting engineer, New York; Charles A. Stone, of Stone and Webster, Boston, president, International Corporation, New York; Frank M. Williams, state engineer, Albany, N. Y.

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THE COMMITTEE ON POLICY OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE committee on policy met at the Hotel Belmont, New York City, on Saturday, September 30, 1916, at 1 P.M. There were present: Messrs. Nichols, Woodward, Paton, Humphreys, Cattell, Noyes, Fairchild, Pickering, MacDougal and Howard.

Mr. Paton read an informal report from the committee on delegates to the meetings. This was followed by a general discussion, after which it was moved and carried that in subsequent letters sent by this committee the authorities be asked especially to send men who will take an active part in the proceedings.

Mr. Pickering made a report for the committee on the Colburn Fund. After discussion, it was moved that the report be approved and recommended to the council for adoption.

After discussion, Mr. Noyes moved to recommend to the council the appointment of a committee of seven on grants for research which shall be authorized to apply the income of the research funds of the association and that such committee be appointed by the president with the advice of the committee on policy.

The treasurer reported for the committee on investments. It was moved and carried that this report be presented to the council with the recommendation that it be published.

The treasurer made a verbal report on certain features connected with the funds of the association and offered to keep in the safety vault of the Carnegie Institution the bonds and other financial papers of the association in charge of two trusted accountants whom, by way of partial compensation, he proposed personally to make life members of the association. This offer the committee accepted with thanks.

Mr. MacDougal reported verbally on the San Diego meeting of the Pacific Division and a long discussion on the welfare of the association ensued.

Mr. MacDougal moved the appointment of a subcommittee of three to consider the relation of the association to scientific organizations in general, to report to the committee on policy in December. Carried. The chairman appointed as this committee, Messrs. MacDougal, Paton and Fairchild.

Mr. Humphreys brought up the question of the publication by the association, or under its auspices, of a distinctly popular journal.

This was discussed at length by Mr. Noyes and certain other members.

Mr. Noyes moved the appointment of a subcommittee of three to report at an early date on this matter. The chairman appointed as this committee, Messrs. Humphreys, Hale and Stratton.

[At this time the committee adjourned for dinner and reconvened at 8 P.M.]

The permanent secretary read a letter from Mr. George E. Hale, chairman of the National Research Council of the National Academy of Sciences, inviting the association to appoint a committee of three to confer with the committee of three of the National Research Council to discuss measures by which the American Association for the Advancement of Science may cooperate in the research council's work.

On motion, it was moved that the president of the association appoint a committee of three, including himself, to act in this manner. On account of its urgency, it was considered desirable to take this action without awaiting the meeting of the council. Carried.

Mr. Noyes reported that the committee appointed from the National Research Council for this purpose consisted of Messrs. Welch, Conklin and Noyes.

Messrs. Charles Baskerville and N. L. Britton, of the local executive committee, appeared by invitation and Mr. Cattell made a statement of arrangements for the New York meeting.

On motion, the committee expressed its approval of scientific exhibits at the New York meeting.

On motion, it was recommended that a member of the local executive committee cooperate with the permanent secretary in preparing the Preliminary Announcement and in arranging the program.

On motion, the nomination of Dr. Henry M. Howe, as vice-president of Section D, in place of Dr. Elmer L. Corthell, deceased.

The matter of "members emeritus," discussed at some length at the Columbus meeting and referred to the committee on policy for recommendation, was brought up by the permanent secretary. It was moved and carried to report to the council that, in the opinion of the Committee on Policy, the Jane M. Smith Fund will care for the most deserving of the cases of the kind under consideration in the council discussion.

At 10 P.M., it was moved and carried to adjourn until Tuesday, December 26, immediately after the meeting of the Committee of One Hundred on Scientific Research at the Hotel Belmont.

L. O. HOWARD,
Secretary

SCIENTIFIC NOTES AND NEWS

ON nomination of the Sectional Committee of Section G (Botany) of the American Association for the Advancement of Science, Dr. C. Stuart Gager, director of the Brooklyn Botanic Garden, has been elected vice-president and chairman of Section G for the coming New York meeting in place of Dr. Thomas J. Burrill, deceased. On similar nomination by Section D (Engineering), Dr. Henry M. Howe, past-president of the American Institute of Mining Engineers, has been elected vice-president and chairman of Section D for the New York meeting, in place of Dr. Elmer L. Corthell, deceased.

BY vote of the board of trustees a bronze bust of Professor Thomas Chrowder Chamberlin will be placed immediately in Rosenwald Hall, University of Chicago, thus recognizing his long service to the university. Dr. Chamberlin has been professor and head of the department of geology since resigning the presidency of Wisconsin University in 1892.

DR. HERMANN VON IHERING has resigned the directorship of the Museu Paulista, São Paulo, Brazil. His present address is Joinville-Hansa, Estado de St. Catharina, Brazil.

THE health of Professor James H. Kemp, head of the department of geology of Columbia University, has much improved. The trustees of the university have, however, granted him a further leave of absence for one year.

DR. ROBERT W. LOVETT, professor of orthopedic surgery at the Harvard Medical School,

has been appointed by the New York State Health Department to inaugurate a state-wide plan in cooperation with local authorities for the after care of patients who have suffered from infantile paralysis.

Dr. R. K. Nabours, professor of zoology in the Kansas State Agricultural College, has recently returned from a trip around the world. Some time was given to further study of the Karakule sheep situation in Russia and Turkestan. He reports it unlikely that any live stock can be transported from these countries for scientific or other purposes during the war.

Dr. R. Tait McKenzie, head of the department of physical education at the University of Pennsylvania, has returned after a year's leave of absence at Aldershot Military Camp and Hospital, of England, where he had charge of training convalescents. He was commissioned major in the regular army of the British Empire.

Dr. R. W. Shufeldt, of Washington, D. C., has been selected to take charge of the department of wild flowers in *The American Forestry Magazine*, beginning with the November number.

Dr. S. W. Williston, professor of paleontology in the University of Chicago, lectured before the Science Club at the Kansas State Agricultural College, on September 23, on "Some Principles of Evolution." He also addressed the student assembly on certain aspects of progress in education.

Dr. Albert John Cook, D.Sc., formerly professor of entomology in the Michigan Agricultural College, for eighteen years professor of biology in Pomona College and more recently for five years state commissioner of horticulture in California, died on September 29, at the age of seventy-four years.

Professor Foster E. L. Beal, a well-known economic ornithologist connected with the Biological Survey in Washington, D. C., and a civil war veteran, died at his home in Branchville, Md., on October 1, in his seventy-seventh year.

The death is announced of Pierre Duhem, professor of theoretical physics in the University of Bordeaux.

The death in Berlin is announced of Dr. Emil Deckert, professor of geography in the new University of Frankfort. Professor Deckert spent many years traveling in and studying the United States. His book, "Nord Amerika," gives the most complete description of the United States in the German language.

The New England Intercollegiate Excursion has been postponed to October 28. The excursion is to the Blue Hills region of eastern Massachusetts and will be conducted by Professors W. O. Crosby and C. H. Warren of the Massachusetts Institute of Technology.

A step looking to a thorough sanitary supervision of the students of Yale University has been taken in the establishment of the university board of health. Student working and living conditions are to be studied and improved, and all students participating in athletics are to be carefully examined. Dr. James C. Greenway, '00, formerly on the medical attending staffs of the New York Hospital and of the Seton Hospital for Tuberculosis of New York, has been appointed university health officer. The board of health includes Dean George Blumer, of the medical school; Professor C.-E. A. Winslow, Dr. W. G. Anderson, of the gymnasium, and the deans of the undergraduate schools.

The Municipal Reference Library of New York City has completed plans for establishing a public health division on the fifth floor of the Health Department building at 139 Center Street. It will specialize on child hygiene, drugs, food analysis, food inspection, food regulations, food supply, hospitals, health insurance, milk supply, occupational hygiene, school inspection, contagious diseases and vital statistics.

The American Public Health Association will meet in Cincinnati, October 24 to 27. Its membership includes approximately 2,500 health officers of the leading cities in the United States and Canada; the executive officers of most of the state and provincial

health departments; officials of the United States and Canadian government health services; and in addition to these, many bacteriologists, chemists, sanitary engineers, sociologists and laymen interested in public health work. Within the association are six sections, composed of the members who are peculiarly interested in the special phases of public health work. The various interests thus represented are indicated by the titles of sections: Public Health Administration, Laboratory, Sanitary Engineering, Vital Statistics, Sociology and Industrial Hygiene. At the annual meeting there will be a program of meetings for the general association, and in addition, the several sections meet for the presentation of papers and discussions on topics relating to their several fields of work. The Cincinnati executive committee, consisting of Drs. E. O. Smith, J. H. Landis and W. H. Peters, are making arrangements to house and entertain 1,000 delegates to the annual meeting, and 150 delegates to the Municipal Health Officers' Conference of Ohio, which meets at the same time.

The surgeon general of the army announces that preliminary examination for appointment of first lieutenants in the Army Medical Corps will be held early in January, 1917, at points to be hereafter designated. Full information concerning this examination can be procured upon application to the "Surgeon General, U. S. Army, Washington, D. C." The essential requirements to secure an invitation are that the applicant shall be a citizen of the United States, between 22 and 32 years of age at time of receiving commission in the Medical Corps, a graduate of a medical school legally authorized to confer the degree of doctor of medicine, of good moral character and habits, and shall have had at least one year's hospital training as an interne, after graduation. Applicants who are serving this postgraduate interneship and can complete same before October 1, 1917, can take the January examination. The examination will be held simultaneously throughout the country at points where boards can be convened. Due consideration will be given to localities from which applications are received, in order to lessen the traveling expenses of applicants as much as possible. In order to perfect all necessary arrangements for the examination, applications should be forwarded without delay to the surgeon general of the army. There are at present two hundred and twenty-eight vacancies in the medical corps of the army.

The private collection of birds, birds' eggs and mammals made by Mr. George B. Sudworth, dendrologist of the U. S. Forest Service at Washington, has just been acquired by the New York State College of Forestry at Syracuse, as a part of the equipment for teaching and investigation in forest zoology. The collection was made largely in Michigan. It is for the most part composed of bird skins which number over 600 specimens and represent nearly 200 species. In addition there are 84 mounted birds. The collection of eggs includes 75 species, belonging to 226 clutches and making a total of 865 specimens. There is one egg of the passenger pigeon and there are two skins of this extinct species. There are 67 mammals. The total number of specimens amounts to more than 1,600. Such collections are now becoming rare, as most of them have been acquired by the large museums.

The report for the year 1914–15 of the Board of Scientific Advice for India consists, according to *Nature*, almost entirely of isolated summaries of the work done during the year by the several scientific departments and scientific institutions of the Indian government. As most, if not all, of these departments and institutions issue independent annual reports of their own, it is, to say the least, disappointing to find these technical summaries filling the report of a scientific body styled advisory; unless, indeed, the term "advice" be understood in the commercial or notificatory sense as merely indicating the existence in working order of these various departmental instruments of research. The advisory proceedings of the board occupy only thirty-seven lines of the 180 pages of the report, and all the information they afford is that the board accepted the programs of the several scientific departments, but would rather not have them

in so much detail in future; and that it recommends (*a*) that officers attending the next Indian Science Congress should be regarded as on duty, (*b*) that a catalogue of scientific serials prepared by the Asiatic Society of Bengal should be published at the expense of government, and (*c*) that experiments should be undertaken, as requested by the Punjab Veterinary Department, to determine the vitality of rinderpest virus under Indian conditions. *Nature* remarks: "Of any far-reaching advisory purpose, of any great original directive enterprise, of anything in the nature of spontaneous movement, this report shows no record; one looks in vain for any reference to scientific education, or even for a connected account—as contrasted with bald, disjointed departmental summaries—of the general progress of science in India, vital affairs in which a board of scientific advice might be expected to exercise a missionary influence, if not to take a commanding lead. The simple fact is that, so far as the advisory business goes, this Report of the Board of Scientific Advice for India is a document of the *ex-officio* genus; and it can scarcely be otherwise when the president of the board is merely an *ex-officio* hierarch of the Indian Secretariat, instead of being a man of science specially selected for his critical knowledge of scientific affairs."

We learn from the *Journal* of the American Medical Association that as a result of the report on the inexactitude of clinical thermometers, read by Mr. Woog at a recent meeting, the Paris Academy of Sciences appointed a commission to study the question. Mr. Grimbert, the reporter of this commission, believes that it is necessary to prohibit the sale of all thermometers the precision of which is not guaranteed by official control. The war having suppressed the importation from Germany, France depends for her supply on Switzerland, England and the United States, and there has been a considerable rise of price without a corresponding guarantee of precision. According to Mr. Woog, the central pharmacy of the army has been obliged to refuse as much as 80 per cent. of the shipments offered. The French manufacturers have assured the commission that they will soon be in a position to supply clinical thermometers at the same price as those obtained from Germany before the war, and that they are prepared to submit to official control. Furthermore, the director of tests at the Conservatory of Arts believes that it is feasible to reduce considerably the fee paid for testing thermometers.

UNIVERSITY AND EDUCATIONAL NEWS

Under the will of Eckley Brinton Coxe, Jr., late president of the University of Pennsylvania Museum, the university was bequeathed the sum of $500,000 as an endowment fund for the maintenance of the museum, its publications and expeditions. He also bequeathed the sum of $100,000 to the university, the income of which is to be used towards increasing the salaries of professors.

Seth Low, president of Columbia University from 1890 to 1901, and trustee from 1881 to 1914, by his will, bequeathed $15,000 to a cousin and $12,000 to the daughter of his former nurse, half of these sums to go to Columbia University on their deaths. On the death of Mrs. Low several educational bequests became effective. Canton Christian College will receive about $70,000, the University of Virginia, Berea College and the Tuskegee Normal and Industrial Institute will each receive about $50,000. Mr. Low gave large gifts to Columbia University during his presidency, including the sum of $1,200,000 for the erection of the library building in memory of his father.

Beginning with this fall the course of instruction in veterinary medicine at the University of Pennsylvania has been placed upon the same basis as other departments of the university in regard to the length of course, four full years now being required for the professional degree.

At New York University Professor John Charles Hubbard succeeds Emeritus Professor Daniel W. Hering as professor of physics; and

Professor Willard D. Fisher has been appointed professor of economics and director of the graduate division of business administration.

DR. JOHN C. SHEDD, who for the past year has been dean of Olivet College and for seven years head of the physics department, has entered upon his work as head of the physics department of Occidental College, Los Angeles.

DR. M. C. TANQUARY, zoologist on the Crockerland Arctic Expedition, returned to this country early in the summer and has recently been appointed assistant professor of entomology in the Kansas State Agricultural College. Mr. A. H. Hersh, of Princeton University, has been appointed instructor in zoology to succeed Mr. Ray Allen, who has accepted a position in Cornell University.

THE following laboratory appointments have been made in the laboratories of the University and Bellevue Hospital Medical College: P. V. Prewitt, A.M. (Missouri), instructor in physiology; E. R. Hoskins, Ph.D. (Minnesota), instructor in anatomy, and J. L. Conel, Ph.D. (Illinois), instructor in anatomy.

DR. L. V. HEILBRUN has been appointed to an instructorship in microscopic anatomy in the college of medicine of the University of Illinois. Last year he was associate in zoology at the University of Chicago.

DR. HARLAN L. TRUMBULL, instructor in chemistry in the University of Washington, has been promoted to be assistant professor.

DR. FREDERIC A. BESLEY has been appointed professor of surgery in Northwestern University Medical School and a member of the attending surgical staff at Mercy Hospital.

C. F. BURGER has been appointed instructor in plant pathology in the graduate school of tropical agriculture of the University of California at Riverside, and Alfred Free Swain, formerly of Montana State College and of Stanford University, assistant in entomology there. Ralph Patterson Royce, formerly livestock editor of the *Missouri Farmer*, has been appointed instructor in animal husbandry at the University of California Farm.

DR. JAMES E. BELL, instructor in chemistry in the University of Washington, has been called as associate professor to Throop Institute of Technology, Pasadena, Calif., where he will have charge of the work in inorganic chemistry.

DISCUSSION AND CORRESPONDENCE

DIFFUSION VS. INDEPENDENT ORIGIN: A REJOINDER TO PROFESSOR G. ELLIOT SMITH

IN the "crude sketch of views" published in SCIENCE for August 11, 1916, Professor Elliot Smith attempts to discredit a method in ethnology which he regards as dogmatic and to substitute for it another which he apparently regards as critical. The issue is the time-honored one of diffusion *vs.* independent development in culture.

It seems to the writer that the picture of the *modus operandi* of "most modern ethnologists" drawn in the initial paragraphs of Professor Smith's sketch is an altogether erroneous one. Without doubt the writers of the classical period of English anthropology often abused the concepts of "independent origin" and "psychic unity of mankind." Of them may be mentioned Spencer, Tylor, Lubbock, Frazer, Lang. The concept of the diffusion of culture through historic contact was, however, by no means foreign even to these thinkers, although they may have neglected to make sufficient use of it in their theoretical constructions. Tylor, in particular, was thoroughly conversant with the problems and manifold difficulties involved in the phenomena of cultural diffusion. As to the modern ethnologists, it would be hard indeed to mention one who has not at some time of his career grappled with the problem of diffusion *vs.* independent development, in material culture, art, religion, social customs. Nor is there one who in his interpretative attempts would make use of the concepts of "psychic unity" and "independent origin" to the exclusion of those of "diffusion" and "historic contact."

On the other hand, a school of thinkers has arisen within relatively recent years, who, following in the lead of Ratzel, have, however,

gone much further, and try to elevate the concept of diffusion to a universal interpretative principle of cultural similarities. "This school is usually associated with the name of Graebner, while among its other adherents, to a greater or less extent, may be mentioned Foy, Ankermann, Schmidt and, in the most recent period, Rivers. While there may be little in common between the work of these men and that of the classical English anthropologists there is, however, one significant similarity in the method pursued: both schools of thinkers seize upon one of the two possible modes of accounting for cultural similarities and proceed to ruthlessly apply it in all instances. In the one case as in the other, then, the method is dogmatic and uncritical."

Having apparently embraced the articles of the Graebnerian faith, Professor Smith sees nothing in the concepts of "independent development" and "psychic unity" but "childish subterfuges" and even "a fetish no less puerile and unsatisfying than that of an African negro." This curiously detached attitude the professor attempts to justify by appealing to the testimony of history and of psychology. "The teaching of history," he asserts, "is fatal to the idea of inventions being made independently. Originality is one of the rarest manifestations of human faculty." As to psychology, we read:

> Nor does it appear to have struck the orthodox ethnologist [here again some names would be most welcome] that his so-called "psychological" explanation and the meaningless phrase "similarity of the working of the human mind" run counter to all the teachings of modern psychology. For it is the outstanding feature of human instincts that they are extremely generalized and vaguely defined, and not of the precise highly specialized character which modern ethnological speculation attributes to them.

As against Professor Smith's interpretation of the historic record the writer ventures to submit that the testimony of history proves beyond the shadow of a doubt that independent inventions do occur as well as that originality, while rare in its most pronounced forms, is in a more general sense as fundamental a trait of the human mind as is that of the absorption and assimilation of ideas. What, if not originality, may we ask, the accumulation of the "happy thoughts" of individual minds, could account for the constant improvements in technique and the neat adjustment and coöperation of parts to which bear witness the manufactures of uncivilized man, his traps and snares, his tools, weapons, canoes, rafts, houses and knots? And what is true of material culture applies equally to the domain of ideas. Again, "if the term invention is given a wide application—as in this instance it should—can there be any doubt whatsoever that numerous and independent inventions have occurred of spirits, taboos and other worlds, of modes of navigation, methods of hunting, fishing, warfare, the making of fire, punishments, ceremonies, myths, social customs, etc. Now, it is a matter of common knowledge that among the things, ideas, processes, thus brought into being, there occur numerous similarities, parallelisms—brief, perhaps, but unmistakable—convergences. When, in referring to these, the modern ethnologist speaks of "psychic unity," he is not therefore guilty of that naïve utilization of the concept of human instincts so confidently ascribed to him by Professor Smith. Again we must urge the professor to name *one* ethnologist who can be shown to have attributed similarities in cultures to the working of "highly-specialized" human instincts. The "psychic unity" is but a substratum, a universal common denominator, without which the similarities referred to above could, of course, not be expected to occur; but the "psychic unity" is manifested no less in the mechanisms of cultural diffusion than it is in those of independent developments. In neither case does "psychic unity" become an explanatory factor. If there is such a thing as explanation in history, then the complete reconstruction of the historic event is the explanation the ethnologist would demand, in the case of diffusion as well as in that of independent development.

The realization of the equal theoretical status of diffusion and independent development presently resolves itself into the percep-

tion of a difference. From the point of view of ethnological technique the two principles can not be treated in an identical way, for whereas diffusion can be demonstrated, independent development does not, in the nature of the case, permit of rigorous proof. The assertion of independent development always involves the negation of diffusion, a negation based on negative evidence, absence of proof of diffusion. Thus, it could always be claimed that at some time somehow diffusion has occurred. Such a claim would be unanswerable. At the same time it is obvious that the above constitutes a methodologically impossible procedure. A relatively small number of cultural similarities—speaking in particular of primitive cultures—can be referred to diffusion by internal evidence. Such is the case when the similarities brought into juxtaposition are so complex and minute that the probability of their independent recurrence approaches or equals zero. But let us repeat, the number of such instances is small, far smaller than generally alleged, far smaller than one might wish. Outside of these cases there lies the tremendous array of cultural similarities which may have arisen through diffusion or by independent development. In all such cases independent development must be assumed until diffusion is proved or, at least, made overwhelmingly probable.

We need not here enter into a discussion of the highly complicated technique demanded of such demonstrations. Professor Smith voices the conviction that the high pre-Columbian civilization in America "was derived from the late New Empire Egyptian civilization, modified by Ethiopian, Mediterranean, West Asiatic, Indian, Indonesian, East Asiatic and Polynesian influences." Professor Smith does not furnish the proof of his contention; it would therefore be premature to pass judgment upon it. But the author forestalls the character of his proof. We read:

The proof of the reality of this great migration of culture is provided not merely by the identical geographical distribution of a very extensive series of curiously distinctive, and often utterly bizarre, customs and beliefs, the precise dates and circumstances of the origin of which are known in their parent countries; but the fact that these strange ingredients are compounded in a definite and highly complex manner to form an artificial cultural structure, which no theory of independent evolution can possibly explain, because chance played so large a part in building it up in its original home.

It seems from this highly significant and interesting passage that Professor Elliot Smith will base his proof largely on quantitative and qualitative evidence derived from the constitution of the cultural complex itself. The publication of Professor Smith's work, notice of which is given in a footnote, will be awaited with the greatest interest and impatience by his American colleagues; and if his proof withstands the test of their open-minded examination, the critical ethnologist will be the last one to want to lift a stone for the destruction of what would then constitute an invaluable addition to our knowledge of the ancient civilizations of the world.

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SOME OBJECTIONS TO MR. ELLIOT SMITH'S THEORY

TO THE EDITOR OF SCIENCE: In your issue for August 11, 1916, there appeared a very interesting theory as to the origins of the pre-Columbian American civilizations. It is the belief of the writer of that article, Mr. G. Elliot Smith, that the distinguishing characteristics of American cultures (such as pyramidal structures, the use of irrigation canals, the custom of mummifying the dead, etc.) are derived, by means of a "great cultural wave," from the ancient civilization of Egypt. The "cultural wave" is said to have passed from the valley of the Nile into Assyria, thence to India, Korea, Siberia, the Pacific islands and America. The wave is said to have started about B.C. 900.

This theory is important. But there are several serious objections to it:

1. If Mr. Elliot Smith is right in thinking that the American aborigines in Mexico, Peru, etc., used pyramidal structures, numer-

ous irrigation systems, and many customs closely resembling those of the ancient Egyptians because their culture was really an offshoot of the Egyptian culture, how can it be explained that in all pre-Columbian America there was no such thing as a wheeled vehicle? Chariots of various sorts were much used in ancient Egypt, as well as in the intervening areas, yet there is not a shred of evidence to prove that the Indians of America ever knew anything even remotely resembling them. Had the founders of American culture come from an area where wheeled vehicles were known, is it not inevitable that they would have made use of such vehicles during their long journey? Does it not seem that wheeled vehicles would be more useful to them than pyramids, and that therefore they would have been remembered first on the arrival of the wanderers in their new land? It is difficult to believe that the American aborigines were the cultural descendants of a wheel-using people, for wheels, being essentially useful, would inevitably have persisted as a feature of their material culture, had that been the case.

2. In a like manner, one is puzzled by a lack of any ships or vessels of advanced type among the American Indians. Even in Mexico, Yucatan and Peru, where civilization was, in other respects, of a well-advanced type, there were no really complicated vessels before the coming of the Spaniards. On the coast of Ecuador there was found the most elaborate type of boat known to the Indian race. It consisted of a raft of light wood with a flimsy platform on which stood a rude shelter. A simple sail, sometimes even two, was used. Large canoes with sails were also used in Yucatan. Not one of these, however, is worthy to be compared with even the earliest and simplest ships used in Egypt.[1] It is known, of course, that boat-building reached very early a high development in Babylonia, India and China, through all of which the "cultural wave" is said to have passed.

3. Finally, the date B.C. 900 is altogether too late for the beginning of the alleged migration of cultures. If this migration took place at all, it must have left Egypt much earlier than this, for we have the Tuxtla statuette (dated about B.C. 100) to prove that even before the commencement of our era the Maya calendar had already gone through its long preliminary stages and was already in existence in practically its final form. No doubt every one will admit that the period B.C. 900–100 is entirely too short for a "great cultural wave" to roll from Egypt to America in. The year B.C. 1500 is much more likely to be the date needed.

In conclusion, the present writer admits that, despite the three objections here noted (and several others), there is a large amount of seemingly corroborative evidence that tends to support the views of Mr. Elliot Smith. It will, however, be a long time before American anthropologists will be forced to accept these views as final, and many tests, based on physical anthropology, history, archeology, etc., will have to be successfully applied before the Egyptian source of American civilization is finally proved.

PHILIP AINSWORTH MEANS
196 BEACON ST.,
BOSTON, MASS.

[1] *Cf.* Joyce, *S. Am. Arch.*, 1912, pp. 60, 125, and Plate XIII.; Joyce, "Mex. Arch.," 1914, pp. 203 and 300; Beuchat, 1912, p. 651; Pinkerton's "Voyages," 1808–14, Vol. XIV., pp. 407–409; Torr, "Ancient Ships," 1895, pp. 2, 4, 9, etc., and Plate I.; Mookerji, "Indian Shipping," 1912.

RESEARCH FUNDS FOR PHARMACY

TO THE EDITOR OF SCIENCE: On page 230 of SCIENCE the appropriation of $5,000 made by the regents for specific research in engineering is mentioned as the only research appropriation at Wisconsin outside of the agricultural grants. For the sake of completeness you may care to know that several years ago the state legislature made an appropriation of $2,500 for a pharmaceutical experiment station, the first one, and, thus far, the only one of its kind in this country. This entire sum, though small as compared with the agricultural grants, is devoted to research. The department of pharmacy also enjoys the income of the Hollister Fellowship Fund of $5,000

for research work. This amounts annually to between $250 and $300. In addition to this the State Historical Society has a fund of $12,000 from Mr. and Mrs. Hollister for a pharmaceutical library. The income of this fund is not being used for the purchase of books, but for historical research in pharmacy and publication of the results. Temporary grants, such as the sum of $500 for a research fellowship by the Association of Flavoring Mfg. of the United States, I suppose fall outside the field covered by the report of the committee.

EDWARD KREMERS

QUOTATIONS

SCIENCE AND INDUSTRY

THE privy council report on scientific and industrial research, of which we publish a summary this morning, is a very able document. It reveals a firm and comprehensive grasp of the subject. To begin with, it gives an account of the existing institution for promoting industry by science. In the National Physical Laboratory, the Engineering Standards Committee, the Imperial Institute, the Imperial College of Science and Technology, the engineering schools of Cambridge and Oxford, the technological departments of the other universities and the larger technical colleges, we possessed before the war an apparatus which would excite the enthusiastic admiration of native critics if they came across it in some other country where the arts of advertisement are better understood and more efficiently practised. It is true that the apparatus was comparatively young, and the use made of it miserably inadequate to its potentialities and to the need; but that was due to a general failure to appreciate either. It is a mistake to infer that we possessed no means for developing industrial science because a poor use was made of them through conservatism, lack of insight, and the obsession of cheap imports deceptively labelled "free trade." The war has changed all that. It has made manifest the need of applying far more energetically the means we have and of supplementing them, as the present report points out. The outbreak of war found us unable to produce at home many essential materials and articles for carrying it on; and since then it has become clear that the future maintenance of our industries in peace demands a new attitude and new efforts in this field on the part of all concerned. This is the sufficient reason for undertaking the reorganization and development of industrial science now, while we are still at war.

The two main things required are financial support and the cooperation of manufacturers. Of the two the latter is, in our opinion, both the more important and the more difficult to secure. If it is effectively secured, the rest will follow; if it is not, nothing else will be of much use. Our manufacturers have not been wholly indifferent to science. The steel industry of Sheffield leads the world in the application of scientific metallurgy to commercial production. Nowhere do the laboratory and the workshop cooperate more closely or with better results. And in recent years other branches of industry have been making a gradual advance in the same direction. But the great bulk of our manufacturing interests have stood aloof and clung to the old. So have the labor interests, which are still more obstructive to change. The British workman's dislike of novelty and his power of resistance are an insufficiently recognized element in the British manufacturer's conservatism. It is obviously useless to spend money on discoveries and new processes if the attempt to apply them leads to strife. This prospect is enough to deter men who might otherwise be inclined to take up research and experiment in their works, and it must be taken into account. But it is not the chief cause of manufacturing inertia. Nor is the small size of many business concerns, to which the report refers. Small concerns can not undertake large, far-reaching researches of a fundamental order; but that is no reason for general indifference or hostility to research. They can carry on scientific work of a different kind with a direct practical bearing on their own business. Some do, but they are few. In Germany they are many. The notion that works there which

employ a large proportion of scientific experts are all on a gigantic scale is quite mistaken. Even those which are on a gigantic scale were small once; they have become large through applying science. Some small works in this country are highly scientific; some very large ones are exactly the opposite. The chief cause of manufacturing inertia is the mentality of British business men, which is essentially practical and distrustful of ideas. But the shock of war has undoubtedly disturbed them, and there is some prospect of a change. It is essential to success, as the committee admit. "We recognize that unless the generality of British firms can be induced to alter their present attitude we shall have failed profoundly in one of our appointed tasks." Research has hitherto offered no career for able and enterprising young men in this country. So they have not gone in for it, and when a manufacturer did want a man he had to go abroad for him. It was a vicious circle. But we believe that in the new prospect now opening up the committee are right in advocating the policy of increasing the supply of men. The demand will follow.—*London Times*.

SCIENTIFIC BOOKS

The Mechanism of Mendelian Heredity. By T. H. Morgan, A. H. Sturtevant, H. J. Muller and C. B. Bridges. Henry Holt and Company, New York. 1915.

Students of genetics some six years ago learned with lively interest that Professor Morgan had discovered in the fly *Drosophila ampelophila* an example of inheritance parallel to that seen in the well-known descent of color-blindness in man. Substituting red eye and white eye in the fly for normal color vision and color-blindness respectively in man the phenomena were exactly similar. Hitherto no such case in an animal available for experiment had been known. We were aware of several instances, notably that of the moth, *Abraxas grossulariata*, the pigmentation of the silky fowl, and certain others in poultry, canaries and pigeons, in which analogous descents had been traced; but in all these the parts played by the sexes were reversed. From this evidence indeed it had been proved that in the moth and the birds the unfertilized *eggs* are differentiated into two classes, those destined to become females and those destined to become males. Obviously enough it would be inferred from the descent of color-blindness that in man the *sperm* was similarly thus differentiated into two such classes, destined to form females and males respectively, a phenomenon which Wilson and others had cytologically demonstrated in various insects. At this point the matter rested.

With the discovery of the peculiarities of *Drosophila* genetic research has passed into a new phase. The animal breeds rapidly, going through many generations in a year. It is inexpensive to breed, and the families consist of numbers which, relatively to those attainable in most subjects, are enormous. Since it first attracted Professor Morgan's attention it has been found to produce a long and intricate series of factorial varieties, or "mutations" as the authors prefer to call them, differing in the color of eyes and body, the sizes and shapes of the wings, and other respects, the number of these differences being now computed at more than a hundred. Professor Morgan and a band of enthusiastic colleagues set themselves with the utmost zeal to analyze the inter-relations of this mass of factors. Half a million flies have been bred, with the result that the data respecting the genetics of *Drosophila* in quantity now surpass those obtained from any other animal or plant. The advances made are on any estimate many and of quite exceptional significance. That much is certain. If we go further, and accept the whole scheme of interpretation without reserve we are provided with a complete theory of heredity, so far as proximate phenomena are concerned.

We may perhaps best approach the subject by reference to a class of facts with which all investigators are now familiar. Of the factorial differences detected in *Drosophila*, many of the more important were soon shown to be sex-limited, as we used to call it, the "limitation" being to males, just as in color-

blindness and some other sex limited affections in man. From an analysis of the descents of these characters Morgan concludes that such limitation is in reality only a special case of that complete or partial association of factors in their parental combinations which was first recognized as coupling and repulsion. These phenomena may in fact be all one. They are examples of linkage between factors, the second factor involved in the case of sex-limitation being that for sex. The fundamental identity of these linkage-phenomena had naturally been suspected. Difficulty, however, lay in the peculiarity of sex-limitation, that in it the linkage has never been observed to be other than complete. The new theory, as will be seen, represents this distinction in a simple and readily conceivable way, so that we are at once attracted. It may be remarked that linkage is no mere incident of technical genetics. We can readily perceive that it must play a great part in the control of heredity. Close resemblances of offspring to parents and grandparents in features and other attributes are common even in families of mixed races like our own. Such resemblances must depend on the coexistence of multitudes of factors, and could scarcely ever be perceptible if the factors were really distributed at random among the germ-cells. The theory provides a mechanism by which their associations may be governed.

From the beginning it was tempting to interpret the processes witnessed in the maturation of the germ-cells as the visible means by which factors are segregated. Cytologists have shown that when the chromosomes are formed anew from the rested nucleus their number and on the whole their forms are constant for the species. They may thus be regarded as having a permanence or individuality. Further, they consist of pairs, one of each pair doubtless representing the material contributed by each parent, the two contributions having retained their identity through all the divisions and changes which have happened since the original fertilization.

If, therefore, the number of genetic factors were never greater than the gametic or haploid number of chromosomes, we should obviously conclude that each chromosome carried one factor, and the ordinary distribution of factors would be produced by a random allocation of one chromosome from each pair to the set comprised in each gamete. But we know that the number of genetic factors in various types of life greatly exceeds the gametic number of chromosomes and consequently this simple account was discarded as insufficient. At this point we meet the first of the far-reaching suggestions which Morgan offers, namely that all the factors are linked together in groups, and that the number of the independent groups is that of the haploid chromosomes. This number in *Drosophila* is four, and it is claimed that, on genetic analysis, the various factors of *Drosophila* can be proved to be so interrelated as to constitute four linked groups and no more. Before wholly accepting a proposition of such magnitude we naturally entertain a provisional reserve, but it may be at once admitted that all the evidence available is capable of this construction. Among the animals and plants already studied are many in which the factors, apparently subject to no linkage, in number far exceed that of the haploid chromosomes, but Morgan is able to reply with force that the possibility of linkage in these cases has not been exhaustively investigated. Tests of the heterozygotes by breeding with double recessives on a considerable scale provide the only really sufficient method of detecting linkages. Such work (especially in plants) is commonly very laborious and has rarely been carried out. Thus, though the presumption would *a priori* seem to be rather against the view that linkage will be found so abundantly operating even in the familiar examples, the speculation is quite legitimate. That it is extraordinarily promising as offering at least a chance of positive progress must be obvious to all.

But if the factors enter the offspring in linked groups—the chromosomes of each pair representing severally the parental combinations—the formation of new combinations inside any one group must mean that there has

been an interchange or "crossing-over" between the two homologous chromosomes. We know that such new combinations can be formed. Gametes bearing them are produced in all cases in which the coupling or the repulsion—to use the older terms—is not complete. To account for the crossing-over of factors from one chromosome to its mate Morgan appeals to certain phenomena of twisting and interlacing of chromosomes in synapsis, first made prominent by Janssens, who observed them in Amphibia. It is suggested that in the course of this process of twisting the chromosomes may anastomose and again break, exchanging parts of their substance. For those unversed in practical cytology it is not quite easy to judge how far this hypothesis is in accord with observed fact. That twisting takes place in many types, especially Amphibia, is clear; but neither the figures reproduced from Janssens nor the originals from which they are taken—still less the very fragmentary observations of both Stevens and Metz from *Drosophila*—provide more than a slender support for this most critical step in the argument. It is to be hoped that the authors will before long tell us exactly upon what evidence they are here relying.

The formation, then, inside a linked group, of factorial combinations other than those which entered from the parents, is ascribed to crossing-over from one chromosome to its fellow or mate. At an early stage in the work, the curious and very significant fact was observed that in the male no such crossing over took place in regard to the various factors which had been proved to be *sex-linked*. The cytological interpretation of this discovery was ready to hand. In many forms, especially insects, the sperms have been proved to be of two kinds, those possessing an X chromosome, destined to form females, and those without this chromosome, destined to form males. If therefore the X chromosome carries the sex-linked factors—a supposition inevitable inasmuch as these factors are all destined to go into the daughters—and if there is no real mate to the X chromosome, evidently there can be no interchange or crossing-over here. Therefore in the case of sex-limited characters linkage is complete.

On tracing the growth of the theory or group of theories which have been built up on the *Drosophila* evidence the consideration just propounded stands out as the original foundation-stone. It was so introduced in the chief inaugural paper of the series. This "sex chromosome in the male has no mate," Morgan tells us, and consequently no interchange with it takes place.[1]

On reference, however, to the work of Miss Stevens (1908) whose paper is given as authority for the mateless condition of the X chromosome in *Drosophila ampelophila,* we read that she found extreme difficulty in studying the cytology of this creature, but ultimately satisfied herself that there is an unequal pair. The more recent cytological work of Metz relates entirely to the female, but in a note on the male he remarks

> so far as my observations go, they indicate an unequal XY pair in the male, without any additional piece attached to either. Neither my observations nor those of Miss Stevens are conclusive, however, owing to the difficulty of observing the chromosomes in these stages. The question is important for the bearing it has upon the breeding experiments with this fly, and we are doubly unfortunate in being thus far unable to settle it.[2]

In 1913, Sturtevant in introducing the first formal development of the theory of linear arrangement, presently to be considered, repeats that there is no crossing over among the sex-linked group of factors in the male, "since the male has only one sex-chromosome."[3] When we come to the book of 1915 the same authors have an entirely different conception of the cytological phenomena. There are two sex chromosomes in the male, and though as a matter of convention, one of them is represented as different from the other in shape, the reader is very properly told that the distinction has not yet been observed.[4]

[1] *J. Exp. Zool.*, 1911, XI., p. 383.

[2] *J. Exp. Zool.*, 1914, XVII., p. 49, *note.*

[3] Sturtevant, *J. Exp. Zool.*, 1913, p. 44.

[4] In the recent paper of Bridges (*Genetics*, I., 1916) the distinction in shape is stated to be a reality.

Without insisting too much on the point, we can not avoid noticing that this complex web of theory is so exceedingly elastic as to be capable of being fitted to a framework of cytological fact, the converse of that for which it was designed. Still, as some animals are found to have no second heterochromosome the suggestion that such a body, when present, may be inoperative might be offered in extenuation.

Presently we meet, however, a fact which is much more difficult to harmonize with the theory, though constituting one of the most novel and remarkable of the discoveries made in the *Drosophila* work. Not only do the sex-linked factors show no crossing over in the male, but experimental breeding shows that in the male there is no crossing over even of the factors composing the other groups. *Crossing over, in fact, in Drosophila, turns out to be exclusively a phenomenon of the germ cells of females.* This is a genetic discovery of the first magnitude, whatever its ultimate significance, but the cytological interpretation of crossing over must now bear a very considerable strain: for, on the one hand, though the absence of crossing over in the sex-linked characters had fitted well with the belief that the sex-chromosome in the male was unpaired, this chromosome is now admitted to be paired; and on the other hand the characters ascribed to the chromosomes known to be paired turn out to be equally unable to cross over in the male. It is with some surprise that we find neither in the book nor in the material previously published any coherent discussion of the difficulties thus created. If further cytological work shows that the chromosomes of the female twist and anastomose, but that those of the male do not, the chromosomal theories of heredity will receive a very remarkable support. Meanwhile on this part of the subject there is little more to be said.

Recombination then within the limits of a linked group is regarded as a consequence of crossing over, or the interchange of parts between one chromosome and its mate or homologue. This conception, whether well- or ill-founded, has led on to a further and very remarkable speculation. If the factors are carried by the material of the chromosomes, what more likely than that they, or rather the particles severally bearing them, should be arranged in a row, like a string of beads, along the length of the chromosome? The proportion of cross-over gametes might thus give an indication of the actual relative positions of the factors along the chromosome. On this inspiration, the intertwining of two strings of beads providing always the mechanical analogy, the numbers in the experimental families have been carefully studied. The percentage of crossovers is taken to indicate the position of the factors. Where there is no linkage, this percentage is, of course, 50, all combinations occurring in equal numbers. But if two factors AB show 50 per cent. crossing over and both A and B can severally be proved to be coupled to a third factor C, then all three may in reality be members of one linked group, and the fact that in the case of one pair there is 50 per cent. of crossing over may be a consequence of the relative positions of these factors in a linear series. The amount of crossing over can thus be interpreted as an indication of the relative positions of each factor in such a series. Upon this follows the great thesis of the book: that this series is in fact a row of points along each of the four chromosomes, and that the redistributions or recombination of characters can be correctly represented by strings of beads which twist together in pairs, breaking and joining each other at nodes. Whether this conception is sound or not, we accept it as a gallant attempt to move on. No other of equal promise has been offered and we must observe its development with cordiality and respect.

Confronted with a theory of so much novelty and importance, the reader's first desire is to examine the details of the evidence from which it has been deduced. A serious charge lies against the book inasmuch as the material for such an examination is not contained in it. We are provided with a sketch—a vigorous and impressionist sketch—of the facts as the authors see them, but we want a much

nearer view. Pending this, judgment must be suspended. We are told that the breeding numbers prove the factors to be in four linked groups. We would like to take each one separately and follow the proof regarding its linkages. As yet there is no means of doing this. Of the evidence the book avowedly gives illustrative specimens merely, and even the long array of *Drosophila* papers leaves great gaps unfilled. Take the first or sex-linked series. The book tells us that more than 40 factors have been located in it and arranged in order. Respecting the great majority we have no details at all and as to most of the remainder very few. There are, however, six that we can examine in the light of the data summarized by Sturtevant in *Zeits. f. Vererbungsl.*, 1914, the last considerable body of evidence to hand.

The factors concerned, called Y, W, V, M, R, Br, are represented as arranged along the chromosome in such a way that two, Y and W, are at the zero end, two more, V and M, near together at 33.5 and 36.5, and the remaining two, R and Br, also near together at 53.3 and 57.7. The numbers indicate that the members of each set of two are closely linked, for with fair consistency the breeding ratios are those characteristic of close "coupling," namely, nAB : 1Ab : IaB : $n$ab, and of "repulsion" in the form IAB : nAb : naB : 1ab, the value of n being much greater than 1. The relations of Y and W to V and M are also of this kind, the coupling being of course less close. But taking Y with R, W with R, V with R, or M with R, we meet numbers of a very different order, and it is not clear by what system they have been interpreted. For instance, we find the following extraordinary series given,

for *Y* with *R*				
as *repulsion*	342	58	466	19
as *coupling*	235	50	194	56
for *W* with *R*				
as *repulsion*	567	143	697	91
as *coupling*	294	61	175	108
for *V* with *R*				
as *repulsion*	147	147	520	36
for *M* with *R*				
as *repulsion*	430	795	1,716	189
as *coupling*	4,189	93	850	1,033

The numbers in which the new combinations come are then added in each case and set out as percentages of the totals, these percentages being taken as indications of the linear distances between the loci in which the factors are presumed to be. To those accustomed to series of this class, these numbers are so aberrant as scarcely to suggest *prima facie* that they represent Mendelian series at all, and it seems at least improbable that they can be used to calculate percentages comparable with those obtained from the various comparatively normal series by which for instance Y and M, V and Bz,[5] Y and W, or W and M are interrelated. Throughout the experiments indications of differential viability recur, largely masking the true proportions of the classes, but as has been remarked by the authors in reference to certain special cases, the incidence of this differentiation is so irregular that allowance can not be made for it in any consistent fashion. Meanwhile the data look so intractable that a doubt has sometimes arisen whether the account here given may not be a consequence of some radical misunderstanding of the author's meaning.

One is tempted further to ask whether all parts of the several proofs are really independent of each other. In the present state of the evidence only the authors themselves can positively answer this question. They declare that all the factors are proved to be disposed in four separate systems of linkage, but the argument that they are thus arranged contemplates a very great variety of possibilities not obviously included in this scheme. For example, the fact that two pairs of gens or factors give 50 per cent. of cross-overs might in the authors' view be a consequence of the location of the two pairs in distinct chromosomes. It may equally be a consequence of the two being in the same chromosome but at the terminal and central positions respectively. It may also

[5] The numbers given for V and Br by Sturtevant are misprinted, 260 standing for 2,660. Thus emended they are fairly normal. The worst examples all involve *R*, and it might be suspected that this was a source of special difficulty, but analogous numerical abnormalities occur also in the "second chromosome" series, nor can any hypothesis of differential viability be readily applied to such figures as those quoted above.

be produced by double or triple crossing over, and in other ways also. Moreover, granting that the factors seem to be related to each other in four systems of linkages, it must next be proved that there is no linkage between members of distinct systems. The evidence of such independence is admittedly meager, and indeed as to the behavior of the factors comprised in the third system we have been told very little at all.

The machinery for dealing with unconformable cases is extraordinarily complete. Besides differential viability we hear of some twelve lethal factors by whose operation certain classes may be extinguished; changes in output with age; a special phenomenon spoken of as "interference" inside single chromosomes; some interaction between chromosomes; even of a factor modifying the normal amount of crossing over, and lastly of an altogether distinct kind of crossing over in the four-strand stage. Can the action of all these processes be severally traced? Can their consequences be distinguished from each other, and especially from those of multiple crossing over? There remain, of course, also the various slips to which all experimental work is liable, such as in this case errors from the overlapping of generations—several times alluded to as a real danger—and others similar which no doubt have been obviated more or less with the improvement in technique. Apart from obscurities of this more superficial kind, is it clear that the series of alternative hypotheses is capable of ultimate analysis? As has been already said, the authors may be able to make such an analysis, but they have not yet offered it to the reader in irrefragable form. Meanwhile the suspicion is unavoidable that, given a conviction that the factors *must* be arranged in rows along four chromosomes, the various interpretations provide rather a method, or perhaps we should say alternative methods, by which the facts can be reconciled with the hypothesis, than a proof that this hypothesis is correct.

Ever since the discovery of systems of linkage it has not been in dispute that several factors, perhaps all, are arranged in some ordinal system or systems. We are dealing with phenomena of *linkage*. The hypothesis of reduplication was offered as one way in which the processes could be logically represented, at least in plants. It is admittedly a very crude conjecture, but it has the merit of being noncommittal and applicable to units of various magnitudes. So much may be remarked in parenthesis; but the critical point now is whether in the various forms of life the number of independent factors, or systems of factors, is or is not greater than the haploid number of the chromosomes. The determination of this question all students of genetics will now await with keen interest.

In all the various parts of the subject explored, whether the main theory prove ultimately to be truth or fallacy, there can be no doubt as to the extraordinary value of the *Drosophila* work as a whole. Of the discovery that may perhaps come hereafter to be regarded as the most illuminating of all—the phenomenon of "non-disjunction"—we have still to speak. The exploration of this group of facts has been made by Bridges, who, since the brief note contained in the book, has published in *Genetics* a detailed account of his experiments. With this publication it must be admitted we are lifted on to something like solid ground. Hitherto amidst all that cytology has contributed, in one respect only has it been found possible to connect quite positively cytological appearances with somatic characters. That in certain forms of life sex is connected with the X chromosome is the one unambiguous fact.

To this Bridges now adds evidence of a new and very remarkable kind. In crosses between females with recessive eye color and normal "wild" males, the daughters normally resemble the father and the sons the mother. As exceptions, "matroklinous" daughters are produced, that is to say in this case with eyes of the recessive color. It was argued *a priori* that such a result might be reached if the *two* X chromosomes of the female were by some chance together passed into an ovum and that ovum were fertilized by a Y-bearing sperm. Such a zygote would be female by virtue of the

two X chromosomes. But for this it would have been male, for it is fertilized by the sperm normally destined to males. Since also all the dominant sex-linked factors possessed by normal males are borne by the sperm normally destined to daughters, the sperm that the exceptional daughter receives is recessive, and therefore these daughters are matroklinous. It follows as a corollary from this argument that fertilization might take place between ova bearing no X at all and a sperm bearing X, and it is said that such a class has been actually recognized as consisting of sterile males. Once the matroklinous daughter has appeared, by breeding from her, a complex variety of consequences may be expected, all deducible from the *a priori* analysis. In the breeding experiments, apart from certain numerical aberrations still unexplained, these have now all been realized experimentally.

Cytologically also the expected appearances have been found—in the sense that egg cells of the "exceptional" females have been seen to contain three instead of two of the chromosomes which the authors now agree are the heterochromosomes. Moreover, from an XXY female it should be possible to breed an XYY male and the two in combination may lead to forms with XXYY, and figures are given showing that these also have been produced and cytologically demonstrated. No one can doubt that this is a very fine achievement. Though still sceptical as to the adequacy of the theory of cross-overs and especially of the soundness of the arguments by which the factors are assigned to serial positions in the chromosomes, it is difficult to see how we can deny that the sex-linked characters have some very special relation to the sex-chromosomes.

In our present ignorance of the nature of life we cannot distinguish cause and effect in these phenomena and it is not possible to attach any satisfactory meaning to the expression that the sex-linked factors are "carried" by a chromosome, but if any one wishes to describe the association of the phenomena in that way there is nothing to forbid him. The properties of living things are in some way attached to a material basis, perhaps in some special degree to nuclear chromatin; and yet it is inconceivable that particles of chromatin or of any other substance, however complex, can possess those powers which must be assigned to our factors or gens. The supposition that particles of chromatin, indistinguishable from each other and indeed almost homogeneous under any known test, can by their material nature confer all the properties of life surpasses the range of even the most convinced materialism. Hence it may well be imagined that even if cytologists decide that in synapsis there is no anastomosis and no transference of material, the effective transference of the gens may occur. The transference may be one of "charges." Perhaps even we might profitably consider whether the chromosomes may not be thrown up, and the gens grouped along their lines by the interplay of the same forces.

Though as must frankly be admitted the *Drosophila* work is on the whole favorable, and in certain respects strongly favorable, to the view that all segregation is effected at the reduction division, it may be well to remind the workers in this field of the phenomena which are inconsistent with that conception. There are, of course, the old difficulties that if the chromosomes play this prerogative part we should expect some broad consistency between their differentiation and that of the forms of life, and we should not anticipate that they would be capable of great irregularities of number and behavior. But apart from these there remain the perfectly authenticated instances not merely of somatic differentiation in regard to Mendelian characters, but the whole range of bud-sports and chimæras of various kinds, and lastly the indubitable evidence that the male and female sides of the same plant may have distinct genetic properties. Such facts, to be sure, are no indication as to the powers of chromosomes, but they are a strong indication that the reduction process is not the only moment at which segregation may be effected. Presumably the advocates of chromosomal views would admit that these are exceptions, but still they are exceptions of a most significant kind. Conceivably we may

be led to the conclusion that there is some radical distinction between plants and animals in these respects.

Many matters of importance are treated in the book, especially the vexed question of the nature of "mutations," to which no justice can be done here. All that can be now attempted is an outline of the essential discoveries. To some it may seem that the disposition of this article is towards undue scepticism. To doubt the theory of cross-overs, for instance, at this date is almost in effect to "draw an indictment against a nation," which we know on high authority is an impossible task. Let it then be explicitly said that not even the most sceptical of readers can go through the *Drosophila* work unmoved by a sense of admiration for the zeal and penetration with which it has been conducted, and for the great extension of genetic knowledge to which it has led—greater far than has been made in any one line of work since Mendel's own experiments.

W. Bateson

PROCEEDINGS OF THE NATIONAL ACADEMY OF SCIENCES

The ninth number of Volume 2 of the *Proceedings of the National Academy of Sciences* contains the following articles:

The Mechanism of Diffusion of Electrolytes through Animal Membranes: Jacques Loeb, Rockefeller Institute for Medical Research, New York. For the diffusion of certain electrolytes through animal membranes there is required besides the osmotic pressure a second effect called the "salt effect" upon the membrane. This consists probably in an ionization of the protein molecules of the membrane.

The Rotation and Radial Velocity of the Spiral Nebula N.G.C. 4594: Francis G. Pease, Mount Wilson Observatory, Carnegie Institution of Washington. The radial velocity is + 1,180 km., in good agreement with the value found by Slipher. The linear velocity of rotation at a point 2 minutes of arc from the nucleus is over 330 km.

A Simple Method for Determining the Colors of the Stars: Frederick H. Seares, Mount Wilson Solar Observatory, Carnegie Institution of Washington. The method suggested consists in determining the ratio of exposure-times which is necessary to produce photographic and photovisual or more briefly, blue and yellow, images of the same size.

Studies of Magnitudes in Star Clusters, III. The Colors of the Brighter Stars in Four Globular Systems: Harlow Shapley, Mount Wilson Solar Observatory, Carnegie Institution of Washington. It is concluded that in all the clusters examined and probably in all globular clusters the volumes of the bright red stars are very great in comparison with the stars that are fainter and relatively blue.

The Effect of an Electric Field on the Lines of Lithium and Calcium: Janet T. Howell, Mount Wilson Solar Observatory, Carnegie Institution of Washington. Lithium and calcium were examined both for longitudinal and transverse effects.

A Proof of White's Porism: A. B. Coble.

A Contribution to the Petrography of the Philippine Islands: J. P. Iddings and E. W. Morley, Brinklow, Maryland and West Hartford, Conn. Six detailed analyses are given of rocks from Luzon, P. I.

Salt Antagonism in Gelatine: W. O. Fenn, Laboratory of Plant Physiology, Harvard University. The experiments on gelatine support the hypothesis that anions antagonize cations in their effects upon organisms. The hypothesis here developed resembles that of Clowes except that it requires that NaCl should antagonize any electrolyte which has either a strong anion or a strong cation. The point of maximum antagonism is an isoelectric point at which the amount of alcohol needed for precipitation is at a minimum, and the aggregation or amount of precipitation is at a maximum.

Similarity in the Behavior of Protoplasm and Gelatine: W. O. Fenn, Laboratory of Plant Physiology, Harvard University. A close analogy to Osterhout's experiments on the electrical resistance of *Laminaria* is found in gelatine (plus NaOH), if we assume that the effect of time in the *Laminaria* experi-

ments is to increase the concentrations of the salts in the cells of the tissue.

On Certain Asymptotic Expressions in the Theory of Linear Differential Equations: W. E. Milne, Department of Mathematics, Bowdoin College. Formulas more precise than those previously obtained by Birkhoff are given.

On Newton's Method of Approximation: Henry B. Fine, Department of Mathematics, Princeton University. A condition is given under which Newton's method of approximation for computing a real root of an equation, and the extension of this method used in computing a root of a system of equations, will with certainty lead to such a root or solution.

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SPECIAL ARTICLES

THE FUNCTION OF THE APYRENE SPERMATOZOA

SINCE the discovery of the dimorphism of spermatozoa in *Paludina* by von Siebold, 1836, numerous biologists have worked with this strange phenomenon without being able to find a conclusive explanation. On the one side investigators were found, like v. Brunn, who regarded the abnormal type of spermia as abortive elements without any function, whereas others, like Brock and Auerbach, thought it impossible that such typical elements could be produced regularly without being functional. It is well known how a new basis was given to these discussions, when Meves (1903)[1] worked out the details of the spermatogenesis of these two types of spermatozoa. He first proved that the atypical spermia in Prosobranchs contain only a small part of the chromatin of the cell and called them *oligopyrene* spermatozoa. He further discovered a similar dimorphism in some moths, but here the atypical spermia contain no chromatin at all, they are apyrene. Meves could get no evidence regarding the possible function of these elements, but he felt sure that they must have some function and pointed to the possibility of a fertilization by these elements, which would be interpreted as an "Entwicklungserregung." Since then much morphological work upon the structure and development of the atypical elements has been done, especially by Retzius, Kuschakewitsch, Reinke,[2] which, however, does not interest us here.

The interest in the possible function of these elements was awakened again, when the facts about the sex-chromosomes became known and their relation to the other kind of dimorphism of spermatozoa in insects and the mechanism of sex inheritance. The idea was promoted by R. Hertwig[3] that this dimorphism might also be connected with sex-determination and he tried to fit these possibilities into his general ideas of sex-determination, considering the possibility of fertilization by apyrene spermatozoa as a kind of male-producing parthenogenesis. These ideas were the starting point of some work which Popoff[4] did with *Paludina*. But he was unable to prove that the oligopyrene spermatozoa of that snail take any part in fertilization, although they are found in sufficient numbers in the oviduct. The only positive result was that in impregnated snails the oligopyrene spermia degenerate and die much earlier than the normal ones. Lamas,[5] who later studied the fertilization of *Murex,* was also unable to find any such spermatozoon inside the egg. On this point we have only a single positive observa-

[1] Meves, F., "Ueber oligopyrene und apyrene Spermien und ueber ihre Entstehung nach Beobachtungen an Paludina und Pygaera," *Arch. Mikr. Anat.*, 61, 1903. See there the quotations of the previous literature.

[2] Retzius, G., *Biol. Unters.*, N. F., Vol. 12, 13, 14, 1905-1909. Kuschakewitsch, S., "Studien übér den Polymorphisms der männlichen Geschlechtselemente bei den Prosobranchia," *Arch. Zellforschg.*, 10, 1913 (complete literature). Reinke, E. E., "The Development of the Apyrene Spermatozoa of *Strombus tuberculatus*," Publ. 183, Carnegie Inst., Washington, 1914.

[3] Hertwig, R., "Ueber Correlation von Zell- und Kerngroesse, etc.," *Biol. Centrbl.*, 23, 1903.

[4] Popoff, M., "Eibildung bei Paludina vivipara, etc.," *Arch. mikr. Anat.*, 70, 1902.

[5] Lams, H., "Recherches concernant le dimorphisme des élements sexuels chez le Murex," *Ann. Soc. Med. Gand.*, 89, 1910.

tion given by Kuschakewitsch.[6] He found in the eggs of a prosobranch, *Aporrhais,* twenty minutes after fertilization, an oligopyrene spermium besides a typical one. Even taking it for granted that this is not an accident of sectioning, the facts are not yet convincing. And Kuschakewitsch himself is indeed rather skeptical and does not want to draw far-reaching conclusions. It might be mentioned that we had formulated a hypothesis[7] of sex-determination on the basis of such a process which, however, we have since abandoned. The latest work on these questions, by Reinke (*l. c.*) finally reached one positive, but rather discouraging result. He finds in *Strombus* that the atypical spermatozoa never reach the receptaculum seminis but degenerate and are surrounded by a capsule. The simultaneous work of von Kemnitz[8] also failed to attain positive results, although the fact is of interest that the hermaphroditic prosobranch *Valvata* exhibits no dimorphism of spermatozoa.

As far as we know, only once has a real experiment been performed to test the function of the apyrene spermatozoa. R. Hertwig[9] started from the hypothesis that fertilization with apyrene spermia is comparable to the parthenogenesis and produces males. Therefore he crossed two species of moth, *Pygæra anachoreta* and *curtula,* which are known to produce apyrene spermia. If fertilization could occur by these latter sex-cells, the offspring, supposedly the males, ought to exhibit only maternal characters. The results were entirely negative, both sexes in F_1 being intermediate in regard to the characters of the parental species.

[6] Kuschakewitsch, S., "Zur Kenntnis der sogenannten wurmförmigen Spermien der Prosobranchier," *Anat. Anz.*, 37, 1910.

[7] Goldschmidt, R., "Kleine Beobachtungen und Ideen zur Zellenlehre," I., *Arch. Zellforschg.*, 1910.

[8] V. Kemnitz, G., "Beiträge zur Kenntnis des Spermatozoendimorphismus," *Arch. Zellf.*, 12, 1914.

[9] Hertwig, R., "Ueber den derzeitigen Stand des Sexualitaetsproblems, etc.," *Biol. Centrbl.*, 32, 1912.

We are now able to state a few experimental facts in regard to the apyrene spermia of moths which have been noticed in connection with some other work. The first question is are the apyrene spermia able to fertilize an ovum or to cause development? Some answer is given by the following facts. In my experiments on intersexuality in the gipsy-moth an almost complete series of intersexual males was produced in 1915 showing every stage from a male to a female. Now up to a certain degree of intersexuality these individuals behave sexually like males and succeed in mating with the females. For many years we have known that such intersexual males of a low grade are completely fertile, and the eggs fertilized by them develop normally. It was of course of extreme importance for our work to breed offspring from the higher grades of intersexual males and they were therefore all mated, obviously to the limiting type, which was still male enough to perform the mating. All matings were certainly normal, as every female laid a normal egg sponge, which is only done after a successful mating. From the eggs fertilized by low-grade intersexual males the normal percentage of caterpillars hatched, as in previous years. But from egg batches, fertilized by somewhat higher intersexual males, only a few caterpillars hatched, the rest of the eggs being unfertilized. The numbers were for four cultures 3, 3, 2, 3 caterpillars, the egg batches containing between 100 and 300 eggs. Finally, in the egg batches laid after mating with the highest type of intersexual male, which was still able to mate, not a single egg developed. Now in studying the sex glands of these males we found that in low grade intersexuality they contained normal sperm bundles, but in the higher forms of intersexuality the entire gonad was filled with giant bundles of apyrene spermatozoa. The intermediate forms, which gave a few fertile eggs, were unfortunately not examined. We think it not unsafe to conclude from these facts that apyrene spermatozoa can not induce development, even if they enter the eggs, which, however, also seems improbable.

But if the atypical spermatozoa play no part in fertilization, what is then their function? We think we can derive an answer from some experiments carried on during the winter of 1914–15 on spermatogenesis in vitro,[10] an answer which is in full harmony with the above quoted results of other investigators.

In rearing the sperm follicles of the moth *Samia cecropia* in tissue-cultures, we found that in the fall the follicles taken from the pupæ finished practically all their normal spermatogenesis and a follicle with apyrene spermatozoa never appeared. But in January and February the results were quite different. The fresh material already contained many degenerating follicles. In the tissue cultures only a very few follicles performed the spermiogenesis, most of them dying after repeated unsuccessful trials to undergo the maturation divisions. These testes, however, already contained many apyrene follicles. Later in February some pupæ were kept in the thermostat for a week. In examining their testes they were found filled with sperm bundles, the great majority of them being apyrene.

In the same experiment it could be shown, further, that the transformation of a sperm-cell into a spermatozoon is caused by the physical condition of the follicle membrane and can be produced artificially to a certain degree. Now one of the main characteristics of the development of the apyrene spermatozoa is the production of caryomerites from the chromosomes and their further degeneration. The same phenomenon has been produced by Conklin[11] in the cleavage cells of *Crepidula* by changing the osmotic conditions of the surroundings. Combining these facts, we reach the conclusion that a definite change in the physical properties of the follicle membrane forces the sperm cells within, physically, to undergo definite atypical changes, which lead to the formation of an apyrene spermium. This process is therefore nothing but the expression of a reaction, necessitated by the physico-chemical properties of the sperm-cell on which the abnormal surroundings act; a reaction produced by abnormal conditions, a teratoma, a lusus naturæ. The typical form of the abnormal sex-cells for a given species is as much necessitated by the specific substratum as the typical form of a plant-gall. The apyrene spermatozoon is a functionless reaction-product.

The results derived from the experiments with intersexual animals are in harmony with this conception derived from the study of tissue cultures. It is well known that the chemical properties of the hemolymph in insects change during metamorphosis in connection with histolysis, and the entire metabolism is put on a different basis, as Weinland[12] proved. In the case of the pupæ of *Samia* it is easy to observe, without going into chemical details, that the blood in old pupæ which produce the atypical spermatozoa has very different properties from those in the young. On the other hand, the work of Steche and Geyer[13] has shown that in the gipsy-moth the chemical characters of the blood are very different in the male and female sex. Hence it might reasonably be expected—tests are going to be made—that in intersexual individuals, where every single character is intermediate to a definite degree between the two sexes, the blood is also different from the normal blood, thus producing in the case of intersexual males those physico-chemical conditions which account for the formation of the apyrene spermatozoa.

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[10] A full account of that piece of work has probably appeared meanwhile in the *Arch. f. Zellforschung*, 1916, under the title: "Einige Versuche zur Spermatogenesis in Vitro." A preliminary notice is found in *Proc. Nat. Ac. Sc.*, I., 1915.

[11] Conklin, E. G., "Experimental Studies on Nuclear and Cell-division in the Eggs of *Crepidula*," *Jour. Ac. Sc.*, 15, 1912.

[12] Weinland, E., "Ueber die Stoffumsetzungen während der Metamorphose der Fleischfliege," *Ztschr. f. Biol.*, 47, 1906.

[13] Geyer, K., "Untersuchungen über die chemische Zusammensetzung der Insectenhaemolymphe," *Ztschr. wiss. Zool.*, 105, 1913.